PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, and Labours,

OF THE

INGENIOUS,

IN MANY

CONSIDERABLE PARTS OF THE WORLD.

VOL. LXV. For the Year 1775.

PART I.

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MDCCLXXV.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume. And this information was thought the more neceſſary, not only as it had been the common opinion, that they were publiſhed by the authority, and under the direction, of the Society itſelf; but alſo, becauſe ſeveral authors, both at home and abroad, have in their writings called them the *Tranſactions of the Royal Society*. Whereas in truth the Society, as a body, never did intereſt themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

At a COUNCIL, January 28, 1773.

Reſolved, That after Volume LXII. the *Philoſophical Tranſactions* be publiſhed twice in a year; the firſt publication to be of the months of November and December of the preceeding year, and January and February of the current year, as ſoon as may be after February, under the name of the "firſt part" of the volume: and the ſecond publication to be of the remaining months unto the receſs of the Society, as ſoon as may be after the receſs, under the name of the "ſecond part" of the volume.

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PHILOSOPHICAL TRANSACTIONS.

I. *Extract of a Letter from Dr.* John Ingenhouſz, *F.R.S. to Sir* John Pringle, *Bart. P. R. S. containing ſome Experiments on the Torpedo, made at* Leghorn, *January* 1, 1773 *(after having been informed of thoſe by Mr.* Walſh). *Dated* Saltzburg, March 27, 1773.

Redde, Nov. 10, 1774.

AS I could get no torpedos alive to my lodgings at Leghorn, I hired a fiſhing veſſel, called a *tartana*, with eighteen men in her, and went out twenty miles to ſea, where the bottom is muddy, and where thoſe fiſh are chiefly to be found. We caught five; of which, four were about a foot in length, and the other of a ſmaller ſize. Before the nets were taken up, I charged a coated jar by a glaſs tube, and gave a ſhock to ſome of the ſailors; who all told me, they felt the ſame ſenſation as when they touched the torpedo. Thoſe people acquainted me, that this animal has but very little force in winter, and cannot live a long time out of the water. As ſoon as the whole quantity of fiſh caught was hauled up upon deck, I put the torpedos immediately into a tub, filled with ſea water, together with two or three other fiſhes, which I found not at all hurt by their company. I took one of the torpedos in my hand, ſo that my thumbs preſſed gently the upper ſide of thoſe two ſoft bodies at the ſide of the head, called (perhaps very improperly) *muſculi falcati* by REDI and LORENZINI, whilſt my forefingers preſſed the oppoſite ſide. About one or two minutes after, I felt a ſudden trembling in my thumbs, which extended no further than my hands: this laſted about two or three ſeconds. After ſome ſeconds more, the ſame trembling was felt again. Sometimes it did not return in ſeveral minutes, and then came again, at very different intervals. Sometimes I felt the trembling both in my fingers and thumb. Theſe tremors gave me the ſame ſenſation, as if a great number of very ſmall electrical bottles were diſcharged through my hand very quickly one after the other. The fiſh occaſioned the ſhock, or trembling, as well out of the water as in it. The ſhock laſted ſometimes ſcarce a ſecond; ſometimes two or three ſeconds. Sometimes it was very weak; at other times ſo ſtrong, that I was very near being obliged to quit my hold of the animal. The torpedo having given one ſhock, did not ſeem to loſe the power of giving another of the ſame force ſoon again; for I obſerved ſeveral times, that the ſhocks, when they followed one another very faſt, were ſtronger at laſt than in the beginning; and this was the ſame when the fiſh was under water as when kept out of it. The preſſure of my fingers, more or leſs ſtrong, did not ſeem to make any alteration in the powers of the torpedo. Applying a braſs chain to the back of the fiſh, where I had put my thumb before, I found no ſenſation at all in my hand, though I repeated the experiment often, and applied the chain for a ſpace of time in which I always perceived a ſtroke(a). This was probably owing to the weakneſs of the fiſh in winter; or, perhaps, becauſe I neglected to put my finger to its oppoſite ſide. Having inſulated myſelf on an electrical ſtand, and keeping the torpedo in my hand, in the manner abovementioned, I gave not the leaſt ſign of being electrified, whether I received a ſtroke from the fiſh or not. The torpedo being ſuſpended by a clean and dry ſilk ribbon, it attracted no light bodies, ſuch as pith-balls, or others, put near it. A coated bottle applied to the fiſh, thus ſuſpended, did not at all become charged. When the fiſh gave the ſhock in the dark, I heard no crackling noiſe, nor perceived any ſpark. When pinced with my nails, it did not give more or fewer ſtrokes than when not pinced. But by folding his body, or bending his right ſide to his left ſide, I felt more frequent ſhocks. Dr. DRUMMOND made theſe experiments with me.

(a) Dr. INGENHOUSZ means, that he felt no ſhock, though he ſaw the animal, by the contortion of its body, give one to the chain. At that time he did not ſeem to know, that though the ſhock would be communicated by a rod of any metal, it could not be ſo by a chain, or where there was the leaſt interruption of continuity.

We diſſected ſome of the torpedos, and found, if I remember well, four very large bundles of nerves, paſſing ſidewards from the head into the two ſoft bodies, called *muſculi falcati*, and diſtributed by denſe ramifications through their whole ſubſtance. Theſe nerves ſeem to terminate in round threads, which ſurround certain cylinders of a tranſparent gelatinous ſubſtance, which ſeems to conſtitute the material part of theſe ſingular bodies that appear to be the reſervoirs of the electric power: theſe cylinders are parallel to each other, and have their direction from the under to the upper ſide of the fiſh. I did not obſerve whether theſe ſoft bodies changed in ſize when the torpedo gives a ſhock; but I ſuſpect they do. I waited for a better opportunity to make a good many more experiments, and repeat, with more care, thoſe already made. But though I have not been fortunate enough to find any other than what is above mentioned, I thought it my duty to inform you of what I have attempted, howſoever incomplete my reſearches have been. And if ever any further opportunity ſhould offer, and in a better ſeaſon, I ſhall not fail to make all the experiments I can think of, for illuſtrating ſo curious and intereſting a ſubject. I am, &c.

II. *An Account of Two Giants Cauſeways, or Groups of priſmatic baſaltine Columns, and other curious vulcanic Concretions, in the* Venetian State *in* Italy; *with ſome Remarks on the Characters of theſe and other ſimilar Bodies, and on the phyſical Geography of the Countries in which they are found. Addreſſed to Sir* John Pringle, *Bart. P. R. S. by* John Strange, *Eſq. F. R. S.*

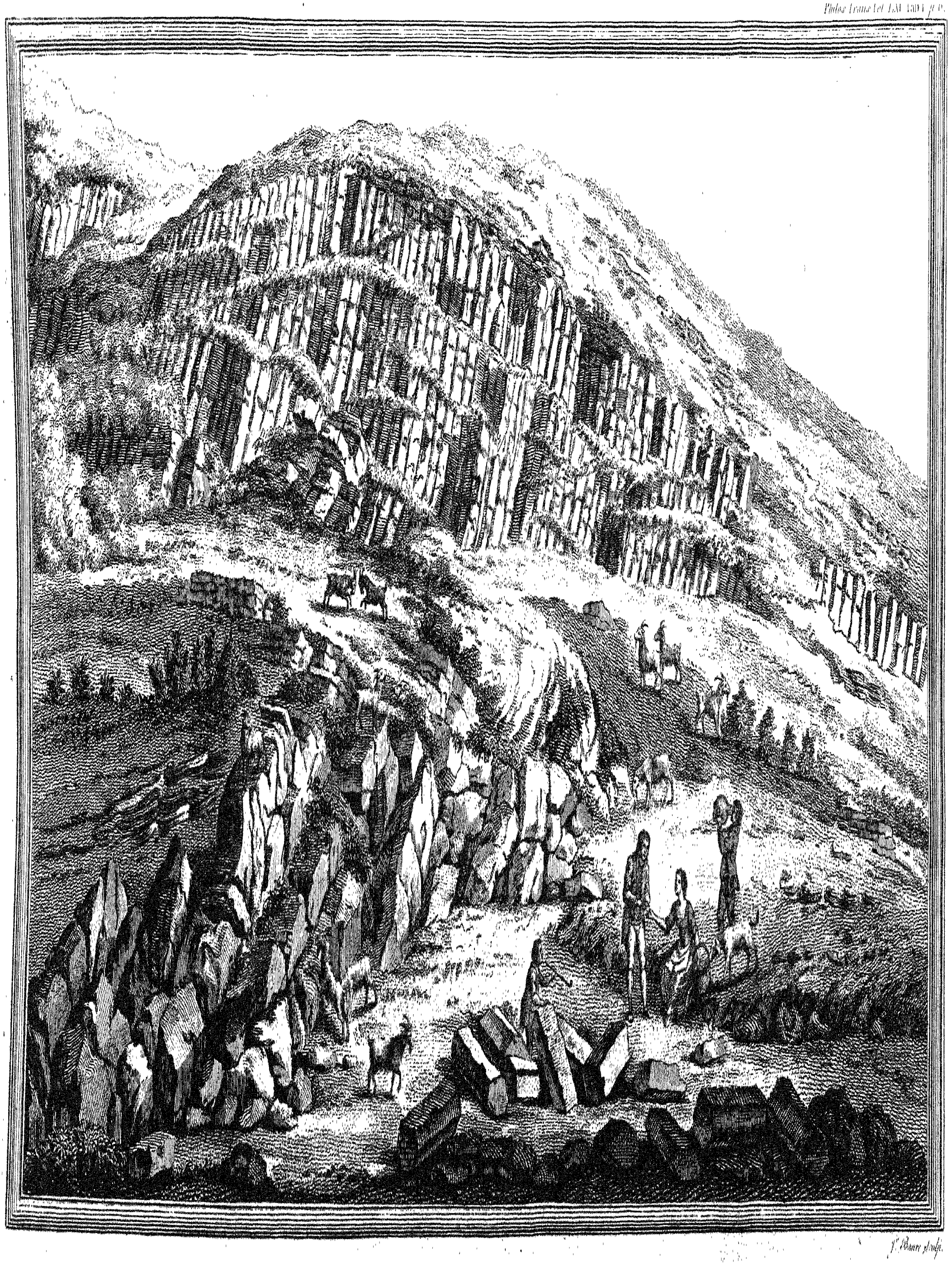
TO SIR JOHN PRINGLE, BART. P. R. S.

Naturalem cauſam quærimus et aſſiduam, non raram et fortuitam.
SEN. Nat Quæſt. L. ii. C. 55.

Redde, Nov. 24, 1774. HAVING had ſome ſatisfaction in the diſcovery of two groups of priſmatic baſaltine columns in the Venetian State in Italy, I thought, that an account of them might poſſibly prove acceptable to you, SIR, and to the other learned members of the Royal Society. I therefore take the liberty of tranſmitting to you the preſent, together with accurate drawings of theſe cauſeways, requeſting of you to communicate them to the gentlemen of the ſociety, ſhould you think they merit their attention. I ſhall, firſt, briefly explain the two drawings; and then add ſuch obſervations as have occurred to me, upon conſidering more particularly the curious originals which they repreſent.

N. 1. Is a topographical view of a part of the ſouth-eaſt ſide of a hill, called MONTE ROSSO, about ſeven miles diſtant, nearly ſouth; from Padua, in the Venetian State in Italy, and a mile to the weſt of ABANO, a village well known, from the celebrated hot baths of that name, and which are ſituated at half a mile's diſtance to the ſouth of it. This view particularly repreſents a natural range of priſmatic columns, of different ſhapes and ſizes, which are placed in a direction nearly perpendicular to the horizon, and parallel to each other, much reſembling that part of the famous Giant's Cauſeway in Ireland, called THE ORGANS, as may be ſeen at Fig. 2. in the weſt proſpect of that Cauſeway, engraved by Vivares, after one of Mrs. S. Drury's excellent deſigns. N. 2. Is a ſimilar repreſentation of the weſt ſide of another baſaltine hill, called IL MONTE DEL DIAVOLO, or the DEVIL'S HILL, near San Giovanni Illarione, alſo in the Venetian ſtate, and Veroneſe diſtrict, about ten miles nearly north-weſt of Vicenza. The priſmatic columns appear to be ranged in an oblique poſition, along the ſide of the hill, not unlike the group repreſented under the rock, marked Fig. 9. in Mrs. DRURY's weſt proſpect of the Giants Cauſeway. This drawing, however, repreſents only a part of the Cauſeway of San Giovanni, which continues along the ſide of a valley, nearly in the ſame manner, to a conſiderable diſtance.

Philos. Trans. Vol. LXV. TAB. V. p. 6.



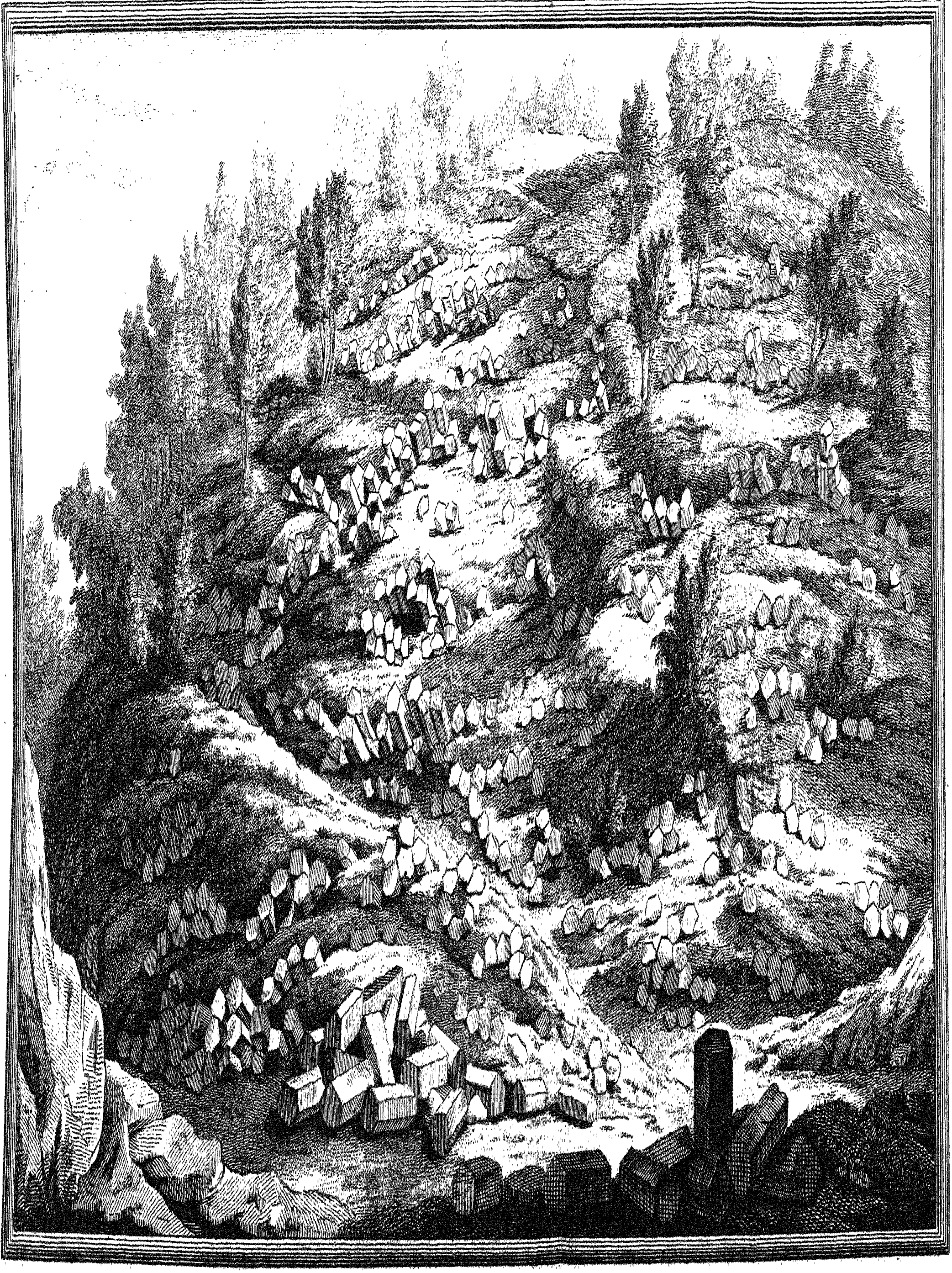
Js. Basire sculp.

diſtance. Though the columns of both theſe hills are of the ſimple, or unjointed ſpecies, *Corneus cryſtalliſatus priſmaticus lateribus inordinatis*, WALLERII, yet they differ very remarkably from each other in many reſpects; but principally in their forms, and the texture and quality of their parts. Thoſe of San Giovanni commonly approach a circular form, as nearly as their angles will permit; which is alſo obſervable in the columns of the Giants Cauſeway, and of moſt other baſaltine groups. On the contrary, thoſe of Monte Roſſo rather affect an oblong or oval figure, as may be more particularly obſerved in the annexed repreſentation of one of them(a). The columns of San Giovanni meaſure, one with the other, near a foot in diameter; nor do they vary much in their ſize; though this is often the caſe in ſimilar groups, and is particularly obſervable in that of Monte Roſſo, whoſe columns ſometimes equal nearly a foot in diameter, while others ſcarcely exceed three inches: the common width of them is about ſix or eight inches. They differ, therefore, very conſiderably in ſize from thoſe of the Giants Cauſeway; ſome of which, as is well known, meaſure two feet in width. I can ſay nothing certain concerning the length of the columns of San Giovanni, ſince they preſent only their tops to view; the remaining parts of them being deeply buried in the hill, and in ſome places intirely covered, as may be ſeen in the drawing. The columns of Monte Roſſo, as far as they are viſible, meaſure only from ſix to eight or ten feet in height;

(a) Fig. 1.

which is alſo a ſmall ſize, when compared with the height of thoſe of the Giants Cauſeway, ſome of which meaſure near forty feet. The columns of the Venetian groups manifeſt, however, all the varieties of priſmatic forms, that are obſervable in thoſe of the Giants Cauſeway, and other ſimilar groups. But they are commonly either of five, ſix, or ſeven ſides; and the hexagonal form ſeems moſtly to prevail, which, if I miſtake not, is alſo remarkable in the Giants Cauſeway, and, as I believe, in moſt others. Nor is there leſs difference in the texture and qualities of theſe columns, than in their forms. Thoſe of San Giovanni preſent a ſmooth ſurface, and, when broken, appear within of a dark iron grey colour, manifeſting alſo a very ſolid and uniform texture; in which characters they correſpond with the columns of the Giants Cauſeway, and thoſe of moſt other baſaltine groups. But the columns of Monte Roſſo are very different in all theſe reſpects. For they have not only a very rough, and ſometimes knotty ſurface; but, when broken, manifeſt a variegated colour and unequal texture of parts. I have broken ſeveral, and have conſtantly found them of this heterogeneous character, and conclude, that the reſt are, as uſual, of the ſame; nor do I apprehend, that, among the whole, there is a ſingle column of an uniform colour and texture, like thoſe of moſt other groups. They are commonly ſpeckled, as it were, more or leſs diſtinctly, and reſemble an inferior ſort of granite, of which Monte Roſſo itſelf is formed, and which ſerves as a baſe to the range of columns in queſtion. It is, in general, not quite

Philos. Trans. Vol. LXV. Tab. II. p. 8.



ſo hard as the Alpine and Oriental granites, and is ſometimes even friable, like the *Saxum granites particulis parum adhærentibus.* ANON. Min. 270. n. 1.; or, *Saxum micaceum quartzoſum ſpatoſumque ſubfriabile.* LINN. Syſt. Nat. tom. iii. p. 76. edit. Holmiæ 1768. LINNÆUS juſtly obſerves, that this ſpecies of granite abounds in France; for I have lately ſeen large tracts of it in the neighbouring provinces of Auvergne, Velay, and Lionnois; and apprehend, that it likewiſe abounds in the Vivarey, Gevaudan, and Sevennes mountains; from the affinity obſervable in the phyſical geography of thoſe countries. But it is equally common in Italy; for beſides Monte Roſſo, the bulk of the Euganean hills in general, of which that is a part, principally conſiſts of it; and theſe hills occupy a conſiderable tract in the plains of Lombardy. It is alſo common in the Tuſcan and Roman States; the mountain cloſe to Viterbo, on the road to Rome, is intirely compoſed of it. The Italians call it *Granitello*, and it much reſembles the *Lapis variolatus* deſcribed by ALDROVANDUS in his *Muſeum Metallicum.* Though partial ſpots of this granite are often friable, eſpecially about the ſurface, yet in general it is very hard; inſomuch, that M. GUETTARD, compares the granites of France with thoſe of Egpyt(b). The columns of Monte Roſſo appear therefore of a different character from any hitherto deſcribed by mineralogiſts, who only mention thoſe of an uniform colour and texture. It has been

(b) Memoires de l'Acad. pour 1751.

obſerved, that the maſs of ſtone in the hill above the Giants Cauſeway, portions of which are repreſented at Fig. 9. in Mrs. DRURY's weſt proſpect, is of the ſame quality and texture with the columns themſelves; which affinity I have conſtantly obſerved between other ſimilar baſaltine groups, and the maſſes to which they belong. It is not therefore extraordinary, that the ſame ſhould alſo be obſervable, between the columns of Monte Roſſo, and the ſort of granite rock on which they ſtand, and with which they ſeem, in a manner, intimately connected. For it is further remarkable, that the maſſes, or ſtrata, of this granite, though irregular, are yet ranged nearly in the ſame direction with the columns above them, as may be obſerved, if I miſtake not, even in the drawing. But the great ſingularity here is, that ſuch a range of priſmatic columns ſhould be found bedded, as it were, in a maſs of granite, and compoſed nearly of the ſame ſubſtance; of which I never yet ſaw or heard any other inſtance. This circumſtance ſeems therefore to render the cauſeway of Monte Roſſo more curious and ſingular than the famous one in Ireland is known to be, from the regular articulation of its columns; the ſame phaenomenon having lately been diſcovered at Staffa, one of the Weſtern Iſlands of Scotland. Different groups of articulated baſaltine columns have likewiſe been obſerved in the province of Auvergne in France; particularly by M. BEOST DE VARENNES(c), at Blaud near Langeac; and by M. DESMARESTS, near *le Mont d'Or*(d). M. SAGE alſo mentions another near St. Alcon, in the ſame province. The Monte Roſſo group is, however, not only curious in itſelf, but very intereſting, on account of the great light it ſeems to throw upon the origin of granites in general, as I ſhall have occaſion to obſerve more particularly hereafter. It is remarkable that the columns, in the two different groups of Monte Roſſo and San Giovanni, preſerve reſpectively the ſame poſition, nearly parallel one with the other; which is not commonly the caſe in other baſaltine groups. For though the principal aggregate, which forms the Giants Cauſeway, ſtands in a direction perpendicular to the horizon; yet other ſmall detached groups of columns alſo appear in the hill above, that affect, by their poſition, different degrees of obliquity. Among the numerous baſaltine hills of Auvergne and Velay, in France, many of which I have lately viſited, and which ſeem to abound in thoſe provinces more than in any other part of Europe, and perhaps of the known globe, nothing is more common, than to ſee the columns of the ſame group lying in all poſſible directions, as irregularly almoſt, as the priſms in a maſs of common cryſtal. Nor is this variety of poſition ſo obſervable in ſingle columns, as in whole maſſes, or ranges of them, which often preſent themſelves, in the ſame hill, diſpoſed in different *ſtrata*, or ſtages, as it were, one above the other, many of which affect very different, and even oppoſite, directions.

(c) Sage Elemens de Mineralogie Docimaſtique, Paris, 1772, 8vo.

(d) De Romè Deliſle Eſſai de Criſtallographie. Paris, 8vo, 1772.

Thus, for inſtance, I have often ſeen a range of horizontal columns placed againſt another, in which all have appeared perpendicular; while a third maſs, adherent to one or other of the former, and perhaps to both, has preſented itſelf, with its columns obliquely diſpoſed. It appears then, that a perpendicular poſition, with reſpect to the horizon, is by no means a characteriſtic peculiar to baſaltine cryſtallizations, as hath been commonly pretended. On the contrary, whole groups frequently occur, that excluſively affect particular degrees of obliquity; as is evident in thoſe of Monte Roſſo, San Giovanni, and many others. Nor is even the horizontal poſition, though leſs common, to be excluded, as I have juſt obſerved; though I have never yet ſeen an entire group of columns ſo diſpoſed. Such a group may, however, poſſibly be found, among the great variety of ſimilar phænomena, which thoſe curious and moſt intereſting provinces of Auvergne and Velay preſent to our notice. The columns of San Giovanni ſeem bedded in a kind of vulcanic ſand, which, in many parts of the hill, intirely covers them; nor do I recollect whether any other ſolid maſſes appear, beſides the columns: theſe, however, probably reſt at bottom upon a baſe of baſaltine rock of the ſame nature. Nothing is more common, in the provinces of France juſt mentioned, than to ſee iſolated baſaltine hills almoſt excluſively compoſed of different layers of columns, which preſent themſelves in ſtages, one above the other, often without any other *ſtratum* between them, reſembling, in ſome meaſure, *ſi magna licet* *componere parvis*, a huge pile or ſtack of cleft wood. Though I do not mean to engroſs this Paper with my obſervations on the Auvergne and Velay baſaltes, which I ſhall refer to a future occaſion, yet I cannot quit the ſubject without adding a few particular remarks, that immediately concern the preſent inquiry. Though the columnar cryſtallization of Monte Roſſo is the only one I have yet ſeen, or heard of, in a maſs of granite; yet other groups of columns have occurred to me in other parts, that are equally of a heterogeneous ſubſtance or texture, though different from thoſe of Monte Roſſo, as well as from the common baſaltes. But I ſhall mention only one inſtance, as the moſt pertinent to my preſent ſubject, in the baſaltine hill, called *Les Rameaux*, near Iſenchaux in Velay. By their form, ſurface, internal colour and texture, the columns of this hill partake of the characters both of the common baſaltine columns, and of thoſe of Monte Roſſo before deſcribed. They approach nearer to the ſub-oval than the circular form; their ſurface is rough, though not knotty; and though they rather incline towards the dark colour, and hard uniform ſubſtance, as uſual; yet, on breaking ſeveral, I found them unequal, both in colour and texture, and ſometimes interſperſed with irregular pieces or patches, as it were, of a heterogeneous hard ſubſtance, which, by its *micæ*, and ſmall rhomboidal cryſtallizations, much reſembles a ſort of granite I have frequently ſeen. The maſs, on which theſe columns ſtand, is of the ſame mixed character; and towards the baſe of the hill, a granite predominates of the ſame nature with that obſerved in the columns. It is alſo remarkable, that granite in general, throughout Velay and the neighbouring province of Auvergne, is frequently intermixed with the baſaltine, and other common vulcanic hills. I have obſerved the ſame in Italy, particularly in the Euganean hills near Padua, and on the confines of the Roman with the Tuſcan State, about Viterbo, Bolſano, &c.; which tracts are alſo moſtly vulcanic. The mountain of Radicofani, and its environs, with thoſe of Aquapendente, are chiefly of the ſame character; and near the lake of Bolſano, by the road ſide leading to Viterbo, is the group of priſmatic baſaltine columns deſcribed by KIRCHER in his *Mundus Subterraneus* (e), and which is the only one in Italy known to me, beſides thoſe of the Venetian State. But the profuſion of baſaltine phænomena in the provinces of Auvergne and Velay is really ſurprizing. In a morning's ride, of about a dozen or fifteen miles, round Iſenchaux, which is the center of the Velay baſaltes, I counted twelve diſtinct groups of columns, in ſo many different hills, detached, and at a diſtance from each other; and as theſe preſented themſelves to my view accidentally, without going out of the way in ſearch of them, it is to be preſumed, that many others, in the ſame neighbourhood, probably eſcaped me. They alſo abound about Puy, the capital of Velay, and ſtill more ſo throughout all Auvergne. Nor is it merely a church, a caſtle, or perhaps a village, as in other countries, that ſometimes crowns the baſaltine hills of Auvergne and Velay. Whole cities are built upon them; a remarkable inſtance of which, among others, occurs in the epiſcopal city of St. Flour in Upper Auvergne, which covers the ſummit of a baſaltine hill, and boaſts a Giants Cauſeway for its foundation. This is more particularly ſeen at the ſouth-eaſt corner of the hill, above the bridge, and on the outſide, under the wall of the town; which circumſtances I mention, in caſe the ſame curioſity ſhould ever lead any other traveller into thoſe parts. As St. Flour is confined to the iſolated ſummit of a hill, and is very cloſely built, the circumference of the walls ſcarcely exceeds much above a mile; but the ſame cauſeway continues from under the town, on the north ſide, to a conſiderable diſtance through the remaining lower part of the ſame hill, upon which the hoſpital of the town is built. Under this hoſpital to the weſt, and by the ſide of a road leading down into the valley from the town, the cauſeway is quite open to view, for a great extent, preſenting the moſt conſiderable ſuit, or continued range, of high columns, that I ſaw throughout the whole tour. Including the continuation of this cauſeway, under the town, and the remaining part of the hill, it forms an aggregate of columns, which, for extent and importance, may almoſt be compared with the famous Giants Cauſeway in Ireland; with this ſignal advantage in favour of the Auvergne group, that it affords the foundation to a conſiderable city. The columns of St. Flour differ alſo from any I have yet ſeen. Among other ſingularities, which I ſhall not conſider at preſent,

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(e) Lib. viii. ſect. 1. c. 9. &c.

I obſerved, that their ſhafts, though of the uſual priſmatic form, are nevertheleſs ſometimes wreathed, or twiſted, like the artificial, round, and ſpiral columns, that are often, though barbarouſly, introduced in buildings. The ſubſtance of theſe columns is, however, of the common ſort, like that of the columns of San Giovanni, and the Giants Cauſeway. The town of Chillac, a few miles above old Brioude, on the river Allier, in Upper Auvergne, is alſo built on a Giants Cauſeway, conſiſting of high, ſtrait, but unjointed columns, which are open to view towards the river. But I ſhall give no further inſtances of this kind at preſent; only muſt beg leave to obſerve, before I quit the ſubject of priſmatic columns, that although no group of them has hitherto been diſcovered in our iſland, yet I am perſuaded, that the mountains of Wales contain one; having, in my tour of that country, obſerved ſeveral large pieces of ſuch columns at Towen, on the ſea coaſt of Merionethſhire, not far from Dolgelthy: particularly about Towen church-yard, where they are uſed as poſts. I could not learn from whence they came, but ſhould ſuſpect, from the character of the adjacent country, that they are found in the mountains towards Dolgelthy, and probably ſomewhere about the famous Cader Idris. For I afterwards obſerved, in my aſcent of that mountain from Dolgelthy, that its predominant maſs appeared to be a ſort of vitrifiable ſtone, ſeemingly of igneous origin, and which I ſhall here take occaſion to obſerve greatly prevails in North Wales, particularly in the three Alpine Counties of Merioneth, Montgomery, and Carnarvon.

Having dwelt a little upon the ſubject of baſaltine organization, I ſhall beg leave to add a few remarks concerning the origin of theſe bodies. The ſyſtematical mineralogiſts, in general, aſſign the ſame common origin to moſt lapideous ſolids, which they ſuppoſe to be generated by depoſition from an aqueous fluid. In whatever manner, therefore, the priſmatic bodies in queſtion are claſſed upon ſuch a principle, no adequate idea can thence be aſcertained concerning their origin, which ſeems manifeſtly different. For ſurely the ſtructure and other phænomena of theſe bodies ſufficiently prove them to be cryſtallizations or concretions of a particular kind, and generated immediately from an igneous fluid: for they are not only peculiar to vulcanic tracts of country; but differ, in every reſpect, from common cryſtals produced from an aqueous fluid. Every one knows, that the latter are formed *ſtratum ſuper ſtratum*, by a ſlow and ſucceſſive depoſition and *juxta*-poſition of parts, as hath been proved ſatisfactorily by CAPPELER, LINNÆUS, and other writers on this ſubject. The ſame mode of generation is more particularly explained by STENO, in his excellent treatiſe *De ſolido intra ſolidum naturaliter contento*. But this mode does not ſeem at all reconcileable with the baſaltine cryſtallizations in queſtion. For however theſe bodies may vary in their texture, none of them, notwithſtanding, afford the leaſt indication of an origin common to other cryſtals; but ſeem rather the effects of ſome intrinſic principle of organization, by which they appear to have been produced ſimultaneouſly,

taneouſly, in a manner, upon the conſolidation of the whole maſs of matter, in which they lie, and with which they conſtantly bear the greateſt analogy, as I have before obſerved. It is further remarkable, that common cryſtals are paraſitical bodies; whereas baſaltine cryſtallizations, notwithſtanding the peculiarities of their figures, rather ſeem to form integral parts of the maſſes to which they adhere; and ſeem to acknowledge, with them, one common and ſimultaneous origin; like the rhomboidal and other cryſtallizations in granites, and other ſimilar vitrifiable compound ſtones. Nor does the common ſlow and limited principle of cryſtallization ſeem at all adequate to ſo great an effect, which ſeems excluſively attributable to an igneous fluid, on the general concretion of which, the organic principle may be ſuppoſed to have operated ſimultaneouſly in a large maſs, and produced theſe bodies in the ſame manner as a linget of metal concretes at once in the mould. No other mode of generation ſeems reconcileable with the phænomena of baſaltine aggregates, as I ſhall more fully prove in the account of thoſe of Auvergne and Velay. Nor do I pretend to determine how ſtrictly this ſimultaneous concretion of parts may be applied to the organization of ſuch bodies, having uſed that expreſſion rather in oppoſition to the ſlow and interrupted ſucceſſion, that, to all appearance, takes place in the formation of common cryſtals. It ſeems alſo further evident to me from the phænomena, that priſmatic baſaltine cryſtallizations, and other regularly figured vulcanic groups (for others have lately occurred to me in Auvergne and Velay, which have never yet been conſidered, and of which I ſhall hereafter give an account) it ſeems, I ſay, evident to me, that ſuch regular bodies have been generated locally, and not in the midſt of thoſe violent convulſions of Nature, which are commonly aſſigned for the origin of vulcanic mountains in general. That the principle of organization, whatever it be, operates locally, in the formation of theſe bodies, appears, I think, ſufficiently evident from the regular diſpoſition and other particular characters of their groups. For notwithſtanding the various directions of the columns, and maſſes compoſed of them, in the different groups, as I have before obſerved; yet, in other reſpects, the greateſt regularity of diſpoſition is commonly obſervable. They form *ſtrata*, which are uniformly organized, diſpoſed in particular directions, and often conſtant in the ſame to a great extent. Theſe *ſtrata* not only manifeſt a paralleliſm between their regularly figured parts, but in their whole aggregates; which often form extenſive horizontal beds, and of an equal thickneſs throughout. This paralleliſm is alſo equally remarkable in groups that are compoſed of many *ſtrata*; as I have particularly obſerved in thoſe of Murat, and the Caſtle Hill of Achon, in Upper Auvergne; in which the columnar *ſtrata* are not only parallel in themſelves, but preſerve, in their poſition, a paralleliſm with the other *ſtrata* of the reſpective groups, which lie in regular ſtages, one above the other: and ſince theſe groups commonly form, in a manner, integral parts of the maſſes, or mountains, in which they are found, and theſe manifeſt alſo ſome affinity in their ſtructure; it ſeems moſt reaſonable to aſſign to both one common origin. The received notions, concerning the mode of origin of vulcanic hills, do not therefore ſeem intirely to correſpond with the phænomena of Nature; ſince naturaliſts generally ſuppoſe, that they have all been thrown up, from the bowels of the earth, by ſubterraneous exploſions, like the Monte di Cenere, near Puzzuoli, in the kingdom of Naples, the iſland of Santorino in the Archipelago, or the ejected entrails of Veſuvius and Ætna. But ſurely this muſt appear a miſtaken notion to any one who compares theſe tumultuary and inordinate aggregates, with the regular vulcanic organizations before deſcribed. For in fact, what does Veſuvius, Ætna, the Monte di Cenere, and ſuch like eructed piles, preſent to us but a heap of ruins, which evidently manifeſt the caſual and extraordinary cauſe, to which they avowedly owe their origin? But this origin ſeems irreconcileable with the regular ſtructures before mentioned, as may perhaps ſatisfactorily appear, from my conſiderations on the particular phænomena, that characterize them. And though it is very poſſible, that ſuch organizations may ſometimes take place upon the concretion of liquified matter thrown up in vulcanic eruptions; yet, however ſimilar they may be, from the nature of their origin, I can hardly imagine they can form other than imperfect and irregular maſſes. For however wonderful the rivers of *lava* of Veſuvius or Ætna may appear to us; they, in reality, are but partial and tumultuary efforts of nature, that by no means ſeem adequate to the production of a Giants Cauſeway, or the baſaltine organizations of Auvergne and Velay, ſeveral of which continue, almoſt uninterruptedly, for many miles. This I have more particularly obſerved in the ſolitary and horizontal baſaltine *ſtrata* that cap the high, though flat, vulcanic hills in Auvergne, and the adjacent part of Velay; as may be more particularly ſeen at the brows of thoſe hills in the reſpective vallies. The celebrated M. DE BUFFON, ſpeaking of theſe hills, ſtyles them *Des plaines en montagnes, qui forment des pays au deſſus des autres païs* (f). They alſo prove the miſtake of ſome naturaliſts, who falſely aſcribing to vulcanic hills in general the ſame origin, as to Veſuvius and Ætna, excluſively aſſign to them alſo the ſame conical or orbicular forms. But, if I miſtake not, the particular and relative characters of baſaltine, and other regular vulcanic organizations of the like kind, contradict a caſual and tumultuary cauſe, and evince the neceſſity of their local origin upon a more ſteady and uniform principle. I ſhall alſo further obſerve, that I never ſaw any certain veſtige of a regular *crater* in any ſpot characterized by ſimilar organizations; which, as I before remarked, form, in general, integral parts of the maſſes to which they adhere, and which alſo frequently manifeſt an analogous ſtructure, however irregular. And ſo far are they from repreſenting the ruinous ſcenes of Veſuvius or Ætna, that they often afford no looſe or iſolated maſſes, except fragments of the columns

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(*f*) Hiſtoire Naturelle, tom. ii. p. 11.

of ſuch groups, which have given way through time. This I have more particularly obſerved in the vulcanic and columnar hills of Iſenchaux in Velay; in which the groups of columns are often ſo united with the body of *lava*, that they form, in a manner, but one ſolid, though figured maſs. Monte Roſſo is preciſely of this character; nor did I obſerve a ſingle column, or fragment even, looſe; thoſe in the drawing being merely introduced to ſhow the forms of the columns. Neither are there here, or in the baſaltine hills near Iſenchaux, any ſigns of a *crater:* on the contrary, theſe hills, as well as Monte Roſſo, are moſtly terminated by regular convex ſummits, that form a ſolid maſs. And that fuſed maſſes ſhould frequently concrete in ſuch a form we need not wonder, if we reflect on the effervescent and expanſive property of fire. The phænomenon of horizontal vulcanic hills is accountable upon another principle, and ſeems chiefly to depend on the ſtate of thoſe hills before their ignition; as I ſhall endeavour to prove in my account of thoſe of Auvergne; and of which the vulcanic hills of the Veroneſe and Vicentine diſtricts afford alſo ſingular inſtances, which I ſhall now conſider. It is difficult to ſay in what ſtate vulcanic hills of a particular and regular ſtructure, like the baſaltine hills, for inſtance, may have pre-exiſted, before their alterations by fire, ſince they afford evident proofs, not only of a liquefaction, but of an intire new organization; by which means all marks of their former characters are totally effaced. Notwithſtanding which, ſince theſe organizations are generated locally, ſome light is often to be had, even in this difficult queſtion, and merely from the phyſical geography of the country, independent of the particular ſtructure of the hills themſelves. For countries have their external characters, according to the nature of the hills that compoſe them; though theſe characters ſeldom form a part of the geographer's inquiries; geography having never yet been formally conſidered, but as ſubſervient to civil hiſtory. I ſhall not enter particularly into this queſtion at preſent; only ſhall obſerve, that the difference, in the external characters of mountains, according to their internal ſtructure, is eaſily ſeen, on comparing, for inſtance, the outward forms of mountains of granite, or other ſimilar vitrifiable compound ſtones, which are of an irregular ſtructure, with thoſe of limeſtone, which are commonly formed in regular *ſtrata*. Signal examples of this are obſervable in the chain of mount Jura, which is excluſively calcareous, and of a horizontal ſummit, and that of the Alps, whoſe higheſt mountains are moſtly of granite, and terminate in pics, pyramids, and other irregularly pointed forms, according to the nature of ſuch mountains. When therefore a remarkable ſimilitude is obſervable, between the forms and diſpoſition of the hills and vallies of a vulcanic diſtrict, and thoſe of other countries of a certain character, that have not ſuffered by fire, it is reaſonable enough, upon the principles before adopted, to ſuppoſe the ſame ſimilitude even in the primary ſtructure and qualities of the former, however they may have been obliterated by the intervention of fire. The vulcanic diſtricts of Auvergne and Velay, as well as thoſe of the Venetian ſtate, afford proofs enough of the truth of this opinion; but I ſhall confine myſelf at preſent to the latter, and particularly to the phænomena of this kind, which I obſerved in the Vicentine and Veroneſe mountains, and which, if I miſtake not, will appear deciſive in the queſtion before us.

Theſe mountains occupy the lower ſkirts of the Alps, on the north ſide of Lombardy, and are partly vulcanic, and partly of limeſtone. They form ſub-diviſions, or lateral branches of the great chain of the Alps, from which they diverge, nearly at right angles, and extend in a ſouthern direction, and parallel with each other, towards the plain. Some of theſe branches are intirely of limeſtone, without any *lava*; others are compoſed of a mixture of both; and others again are excluſively vulcanic. I have rode from the point of one of them near Montebello, in the Veroneſe territory, to Bolca, always upon *lava*, for the diſtance of near twenty miles. It is along this branch, by the ſide of the valley leading to Bolca, and about four or five miles ſhort of it, that the cauſeway of San Giovanni Illarione before deſcribed is ſituated. The whole ſolid maſs of this branch, as far as I could obſerve, is almoſt intirely compoſed of *lava*, which, about the ſkirts and ſurface particularly, is of various kinds. Among others very curious, I remarked ſome, at the foot of the caſtle hill near Montebello, concreted in different maſſes, which, by their extreme hardneſs, heterogeneous texture and colour, very much reſemble an ordinary ſort of porphyry. But I ſaw no granite mixed with the *lava*, in theſe lower limeſtone ſkirts of the Alps, though it abounds ſo much in the neighbouring Euganean hills, as well as in Auvergne and Velay, as I before obſerved. Notwithſtanding the general vulcanic character of this branch of Montebello, its original ſtructure and characters are ſtill very evident, and perfectly correſpond with thoſe of the neighbouring branches, that have never ſuffered fire. For though ſome new igneous modifications have accidentally and partially taken place about the ſkirts and ſurface of this branch; yet in other more internal parts, not only the original horizontal poſition and paralleliſm of the *ſtrata* are manifeſt, but ſmall, though integral parts of thoſe *ſtrata*, here and there, remain unburnt, and ſhow their calcareous qualities, ſtructure, and extraneous contents, perfectly ſimilar to thoſe of the other neighbouring mountains, that have never ſuffered by fire. This I particularly obſerved along the brows, or upper lateral *ſtrata* of the vulcanic branch juſt mentioned, above the valley between Sorio and Montebello. The famous foſſil fiſh quarry at Bolca, ſo well known to all the curious in Europe, is only an unburnt, calcareous, and ſlaty point, or ſide promontory, as it were, of the higheſt part of the ſame vulcanic branch, that deſcends into the valley, from the church and village of Bolca, which are built upon it. This point within unites immediately with the *lava*, forming, in a manner, an integral part of the ſame hill. In other parts again, and particularly at Ronca, alſo in the Veroneſe territory, a few miles to the north-weſt of

Montebello, though the maſs is converted intirely to *lava*, and has evidently concreted from a fuſion, yet the marine foſſil bodies, originally contained in the *ſtrata*, are diſtinguiſhable, and even diſtinct in the *lava*, though variouſly disfigured. Another obſervation I made, and which appears to me very intereſting, is, that moſt of theſe branches, in the Veroneſe and Vicentine territories, whether marine, vulcanic, or mixed, ſtill preſerve nearly the ſame external characters, directions and paralleliſm, excluſive of the trifling alterations produced at the ſurface of the latter, as I before obſerved. It ſeems, therefore, ſufficiently evident, that fire not only operates locally on lapideous ſolids, but often alſo in ſuch a manner as not intirely to deſtroy all marks of their primary organization and qualities, much leſs to alter their diſpoſitions, and the external characters of the maſſes or mountains, they form. And though all traces of the primary organization of theſe maſſes may be effaced by new modifications, yet often ſufficient proofs remain of their former characters, in the forms, direction, and diſpoſition of the mountains they compoſe, as appears from the inſtance I have juſt conſidered, and is ſtill more ſtrongly confirmed by the phænomena of Auvergne and Velay, which I ſhall conſider upon a future occaſion. It does not, therefore, ſeem impoſſible, nor even difficult, to trace the leading original character of a country, though it has ſuffered by fire a new modification of the *ſtrata* that compoſe it. And the vulcanic mountains before deſcribed not only afford evident marks of their having pre-exiſted in another ſtate, but manifeſt alſo plainly their primary qualities and ſtructure, and equally prove, by their particular directions, that they never can have been thrown up fortuitouſly, from the bowels of the earth, like the Monte di Cenere, &c.; but have ſuffered fire *in ſtatu quo*, or locally, without the leaſt appearance of ſubverſion, or change of place. The ſame ſeems alſo very probable of many other mountains, that are purely vulcanic, from the neceſſity of the local origin of their particular organizations, which I have before conſidered; and ſuppoſing even that ſuch mountains manifeſt no internal nor external ſigns of their primitive ſtructures or qualities. From the preceding obſervations it appears, I think, evident, that ſubterraneous exploſions and eruptions are merely accidental phænomena, that are by no means eſſential to the production of all vulcanic mountains, as has been commonly imagined. This notion ſeems to have proceeded from the affinity often obſervable in the qualities and external forms of many ſuch mountains, and thoſe of real volcanos. But I have already obſerved, that there are many vulcanic mountains of a totally different form from the common volcanos; and though their *lava* may ſometimes be ſimilar in its qualities, yet the regular organization of it, in the former, frequently makes a moſt eſſential difference. For, as I before ſaid, what does Veſuvius or Ætna preſent to us but a heap of ruins, which give us not the leaſt idea of the ſtructures to which they belonged? And though they may lead and enlighten the chymiſt, yet they afford but little inſtruction to the phyſical geographer, a conſtant ſameneſs of phænomena occurring every where. Nor have we any foundation, from the external appearance of ſuch mountains, to conclude, that all others, that have ſuffered fire, are of the ſame character. We ſee nothing but a heap of ruins, caſt up from their bowels, and we are apt to imagine, that ſuch inordinate materials compoſe the intire maſs; and analogy, too often ſeducive in ſimilar matters, leads us to conclude the ſame of other vulcanic mountains in general. But I am much inclined to think, that the materials thrown up by burning mountains, are only lodged ſuperficially, as it were, on their ſides; and though they may conſiderably increaſe their bulk, as well as alter their form, yet they do not ſeem to conſtitute the intire maſs of thoſe mountains, as might be reaſonably imagined from their external appearance. For it has been obſerved, both by PADRE DE LA TORRE, M. DE LA LANDE (g), and others, that the inner ſides even of the funnel of mount Veſuvius preſerve manifeſt veſtiges of its primary organization, in regular, parallel, and nearly horizontal *ſtrata*, like thoſe of other common mountains. And does it not appear more than probable from hence, that an original mountain lies under the *lava* of Veſuvius, ſerving in a manner, as its baſe, and which, whatever local alteration it may have received intrinſically, from the ſubtle element, that waſtes its bowels, ſtill maintains its primary undiſturbed ſtructure, like the vulcanic mountains of the Veroneſe territory before deſcribed. It is not therefore likely, that the whole of mount Veſuvius ſhould have been made at ſeveral times, by the earth and cinders flung up out of the mouth, that lies in the midſt of them, the aſhes falling down the ſides of it, like the ſand in an hour-glaſs, as Mr. ADDISON(b) particularly obſerves, and as moſt people are naturally enough apt to think. And however conſiderably the bulk of this mountain may have been increaſed, by the loads of *lava* thrown out at the ſeveral eruptions; yet no great addition ſeems to have acceded to its height, if the obſervation juſt mentioned be true; which, though I have never had it in my power to verify, yet I readily believe; not only on account of the reſpectable authorities above quoted, but alſo from its conſiſtency with other ſimilar and indiſputable phænomena, of which the vulcanic branch of Montebello before deſcribed affords, if I miſtake not, no inconſiderable proof. Nor does it indeed appear agreeable to reaſon, that mountains of great height, where volcanos exiſt, ſhould be entirely vomited up in this manner; ſuch volcanos ſeeming to require a ſolid and permanent channel for the violence and frequency of the effects they produce. It ſeems therefore, that a way or channel only has been opened, by an original effort or exploſion, through mount Veſuvius, which has ſince been deeply buried by the materials ſucceſſively thrown up, from its bowels, in the ſeveral eruptions. And I am inclined to think the ſame of mount Ætna, from its ſuperior height, though I am uncertain whether BORELLI, or any other of its hiſtoriographers, takes notice of any fact, by which this conjecture is proved. The ſame ſeems alſo probable, for ſimilar reaſons, of mount Hecla, the Pic of Tenerif, &c. And if this be true of ſingle or iſolated volcanos, like Veſuvius and Ætna, that carry ſuch ſtrong external marks of having been excluſively produced by the accumulated *lava* of eruptions, much more is it likely to be ſo of others, that are not only conſiderably higher, but form parts of a continued chain, like the volcanos of the Andes, as I imagine. Is it not, moreover, highly improbable, that Chimboroſo, which is one of thoſe volcanos, and the higheſt mountains in the known globe, meaſuring, according to the French academicians, 3220 toiſes, which exceed four Italian miles, allowing 764 toiſes to a mile; is it not, I ſay, highly improbable, that ſuch a mountain ſhould excluſively owe its origin and formation to the accumulated materials of eruptions only? May not the ſame be ſaid of the mountains Antiſana, Kotopacſi, Pichincha, and the other volcanos of the Andes, of which ſome are little inferior in height to Chimboroſo, and conſtitute in general the higheſt parts of that vaſt chain? And though the ſummits of theſe mountains, in all probability, form iſolated points, yet, I preſume, they unite in an uninterrupted maſs below, like other chains of mountains. And if this be true, it is hardly credible, that the maſſes where theſe volcanos reſpectively exiſt, can have been indebted ſolely to them for their origin, or that they can have been fortuitouſly caſt up from the

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(g) Voyages d'Italie, tom. VII. p. 169. 176.

(b) Travels, p. 184.

bowels of the earth like the Monte di Cenere, the iſland of Santorino, &c. If they form integral parts of a continued chain, as it is natural to ſuppoſe, is it not even abſurd to imagine, that they can have had ſuch an origin? Is it not, on the contrary, rather to be preſumed, that channels only have been opened along this chain, by different exploſions, where theſe volcanos reſpectively exiſt; and that the ſides of theſe channels form integral parts of its original ſtructure, as in the caſe of mount Veſuvius before remarked, and which here ſeems to receive the ſtrongeſt confirmation. For however the eruptions of the volcanos of the Andes may have loaded their ſides and ſummits in particular parts; yet ſurely inferior maſſes exiſt of a much prior origin, and whoſe continuity ſufficiently ſeems to prove, that ſuch eruptions are, relatively, only accidental phænomena. This reaſoning is, however, grounded upon the ſuppoſition, that the Andes form a continued chain, like that of other ſimilar mountains; and, as I preſume, they do. From the preceding obſervations it ſeems therefore evident, that whether vulcanic mountains preſerve, or not, veſtiges of their ancient and primitive ſtate, or in whatſoever manner they appear to have been newly organized; few of them ſeem to have been intirely thrown up from the bowels of the earth, like the Monte di Cenere, &c. On the contrary, they moſtly appear to have pre-exiſted in another ſtate; and to have ſuffered by fire only locally, and more or leſs partially, of which I have given ſufficient proofs; or, having afforded only paſſages to exploſions and eruptions, are partly in an original ſtate within, and partly increaſed by new and adventitious materials ſuperadded to their ſurface by ſucceſſive eruptions; as ſeems to be the caſe of the vulcanic mountains of the Andes, mount Veſuvius, and probably of moſt other volcanos of any great height; more eſpecially where they form parts of continued chains. And if ſo few of the extinct vulcanic mountains appear to have been thrown up, from below the common ſurface of the earth, like the Monte di Cenere, &c. nor even thoſe intirely which manifeſt actual volcanos; it ſeems highly improbable, that other common mountains ſhould have had ſuch an origin; as many reſpectable writers (i) have been inclined to think; and ſtill more ſo, that ſuch ſhould have been the ſole origin of all mountains; as a late Italian writer (k) on the theory of the earth has very unſucceſsfully endeavoured to prove. It alſo plainly appears, if I miſtake not, from what has been before ſaid, that the phænomena of recent volcanos are very little calculated to give us much inſtruction about the more curious igneous concretions, and the origin of vulcanic mountains in general; and that a few days tour in ſuch countries as Auvergne, Velay, and the Venetian ſtate are worth a ſeven years apprenticeſhip at the foot of mount Veſuvius or Ætna; where nothing but a heap of uninſtructive ruins, and a ſameneſs of phænomena appear. And ſince our ideas, concerning vulcanic effects, have been almoſt excluſively drawn from recent volcanos, we cannot much wonder if they yet remain ſo imperfect.

(i) HOOKE's Philoſophical Diſcourſe on Earthquakes in his poſthumous Works. RAY's Phyſiological Diſcourſe. RASPE Specimen Globi Terraquei.

(k) MORO de Croſtacei che ſu i Monti ſi trovano.

Having dwelt a little, in the courſe of this paper, on the phyſical geography, and particular vulcanic phænomena of Auvergne, Velay, and the Veroneſe and Vicentine territories; I ſhall beg leave to add a few obſervations of the like nature relating to the Euganean hills; more eſpecially as they produce other vulcanic concretions equally curious, and of a very different character from any obſerved in the provinces before mentioned. The Euganean hills form an irregular group in the plain of Lombardy, about ſeven miles nearly ſouth by weſt from Padua, and extend from north to ſouth as far as Eſte. The moſt conſiderable part of them compoſes an irregular ſort of chain, which extends in the above direction; while other parts are ſeverally detached, and form iſolated mountains about the ſkirts of this chain, particularly on the north-eaſt ſide, towards Abano. The outer ſkirt of the intire group may meaſure perhaps from thirty to forty Engliſh miles. The external characters of this group exactly correſpond with the forms commonly aſcribed by naturaliſts to vulcanic mountains in general; ſince the points of the chain before mentioned, as well as the iſolated members of it, are of various conical, orbicular, and elliptical ſhapes. As this group, therefore, reſts upon a perfect plain, it makes a very ſingular appearance, and exactly anſwers to the following lines of Ovid(l), which, I hope, I may therefore be permitted to inſert, though in a philoſophical paper.

(l) Met. lib. XV.

Extentam tumefecit humum, ceu ſpiritus oris
Tendere veſicam ſolet, aut direpta bicornis
Terga capri; tumor ille loci permanſit, et alti
Collis habet ſpeciem, longoque induruit ævo.

The vulcanic hills immediately round Iſenchaux in Velay affect alſo the ſame forms; but as they are mixed with other hills of a different form, and the country about them is broken and irregular, they do not produce ſo ſingular an effect as the Euganean hills, which ſuddenly riſe from a perfect level. I am informed, that there is a ſimilar, though ſmaller group of iſolated vulcanic hills in a plain of Dalmatia, near Coſſovo; and another group of hills, nearly of the ſame forms, in the county of Down, in Ireland, and called the Mourn Hills; which, like thoſe near Padua, conſiſt, as I am informed, moſtly of granite and *lava*. The Euganean hills have, moreover, a ſuperficial and partial covering of ſlaty and calcareous *ſtrata*, of poſterior origin, and that manifeſt no marks of having ſuffered by fire. Such *ſtrata* ſlightly cap mount Venda, which is the higheſt among theſe hills; though of no very conſiderable elevation, meaſuring only about 252 French toiſes above the Venetian Lagunes, according to abbé Toaldo, profeſſor of aſtronomy at Padua, who lately, at my requeſt, obligingly took its elevation from the obſervatory at Padua. From the *lava* and granite mixed together in the Euganean hills, they bear an affinity with thoſe of Auvergne and Velay; but differ from them by the ſuperincumbent unburnt *ſtrata* of Lime-ſtone. This they call Scaglia, or Scagliola, from its being compoſed of thin ſlaty *ſtrata*, which are of a yellowiſh colour, and contain a few veſtiges of foſſil marine bodies, but no regular bed of them. Sometimes an irregular maſs of marble is found among the Scaglia, and near Arqua particularly, of a moſt beautiful kind, much reſembling the noted Florentine figured marble; with this further addition, that, beſides the ruins it repreſents, it is alſo variegated by frequent dendrites, which, if I miſtake not, are very uncommon in the former. Part of the tabernacle of the great altar in the church at Arqua is of this marble, which alſo takes a moſt beautiful poliſh. If Vaucluſe, near Avignon in France, is become celebrated from the memory of the plaintive and eloquent PETRARCH, Arqua ought ſtill to be more ſo; ſince not only his remains lie there, in a large *ſarcophagus*, of red Veroneſe marble, in the church-yard; but his villa at Arqua is ſtill in being, and preſerves ſome paſtoral and hiſtorical freſco paintings, of himſelf and his LAURA, of no inconſiderable merit. His great armed chair, and the ſkeleton of his favourite cat, are alſo ſtill in being. This villa was his retreat, during his reſidence at Padua, where he was a canon of the cathedral. The common *lava* is not ſo frequent among the Euganean hills as in the provinces of Auvergne and Velay, and ſeldom forms intire hills, which are moſtly of granite, on the ſurface of which, where there is no lime-ſtone, the *lava* is partially and ſuperficially ſcattered, and ſometimes mixed even with the maſs of granite. I have already obſerved, that the priſmatic group of Monte Roſſo is nearly of the ſame ſort of granite with the hill, and is the only one in thoſe parts; the common vulcanic tracts affording nothing ſimilar. Having dwelt ſufficiently on this group, in the beginning of my paper, I ſhall briefly add a few obſervations on the phyſical topography of Monte Roſſo itſelf, with an account of ſome other of its vulcanic productions, not leſs curious than the group of columns before deſcribed.

Monte Roſſo is iſolated from the principal chain of the Euganean hills, of an orbicular form, and meaſures about a mile and a half in circumference at the baſe. It conſiſts principally of gray granite, which is diſpoſed in blocks, and irregularly perpendicular *ſtrata*. I have already mentioned the quality of this granite, and its reſemblance with that of Auvergne and Velay. Towards the ſurface of Monte Roſſo, it appears ſometimes rotten, or friable, and porous, as it were, like a motley kind of *lava*, which I have frequently ſeen. But I was ſurpriſed, on examining the granite maſſes of Monte Roſſo, to find in them pieces of common porous brown *lava*, which did not appear to be caſually lodged there, and of extraneous origin (like the rounded pebbles in pudding ſtone, and other aqueous *ſtrata*) but manifeſtly ſeemed to form integral parts of the maſs itſelf, and to have concreted with it at the ſame time. I obſerved a ſimilar ſort of porous *lava*, but of a black colour, in the granite of the caſtle hill at Monceleſe, near Eſte, at the ſouth eaſt ſkirt of the Euganean hills; and I doubt not but the ſame is common to others. This fact, added to many others, which I ſhall not inſiſt upon at preſent, ſeems ſtrongly to confirm an opinion, which I have long entertained of the igneous origin of granites in general; ſome further proofs of which I ſhall hereafter conſider. A dingy red ochrous earth covers partly the ſurface of Monte Roſſo, from whence probably proceeds the name given to the hill; *roſſo*, in Italian, ſignifying *red*. Much iron ſand alſo abounds here, as it commonly does about other vulcanic and granite mountains or tracts in general. Among the figured concretions of Monte Roſſo, I obſerved a ſmall open perpendicular bank, at the eaſt end of the hill, which preſented a group of a very peculiar ſtructure. It is formed by an aggregate of angular bodies, laterally ranged together, like balſantine columns, but in a horizontal direction, with their tops in front, and prominent, as they are repreſented in the figure(m). Theſe prominencies are of a globoſe form, and made rough by a number of ſmall cryſtallizations, of a parallelipepid figure, that are concreted in the maſs, which is of a yellowiſh colour, and rather friable ſort of vulcanic ſubſtance, inſomuch that I could not ſeparate, or iſolate the bodies ſo far as preciſely to determine their particular form, though a correſpondent continuation of the external angles appears within, and they ſeem to contract a little, pyramidically, like the iſolated body figured(n), and which ſeems to be ſomewhat of the ſame kind, but of a much harder ſubſtance. This is alſo from a part of Monte Roſſo near the priſmatic columns. Though, as I before ſaid, there are very few of the Euganean hills that intirely

(m) Fig. 2. (n) Fig. 3.

conſiſt of common baſaltine, or other *lava*, like thoſe of Auvergne, Velay, and the Vicentine and Veroneſe territories; yet ſome there are, and very curious, which I ſhall briefly deſcribe. Monte Nuovo, or the New Mountain, about a league to the ſouth of Abano, and near Battaglia, is one of them. Though connected with the principal chain of the Euganean hills, by a depreſſed neck, or iſthmus, yet it contains no granite, or lime-ſtone, like the reſt of that chain, but is formed excluſively of *lava*, of various kinds, and different from any *lava* I obſerved about theſe hills. Great part of its ſurface, eſpecially about the top, is very rough, knotty, and ſinuous, and manifeſtly appears to have concreted from a fuſion by fire. The ſkirts and bottoms of it equally prove the ſame, conſiſting moſtly of another ſort of mixed and congealed *lava*, which the Italians call *lava brecciata*, from its reſemblance to the Breccia marbles. It is formed by many broken and irregular fragments, that have been accidentally licked up, as it were, or collected by the *lava*, while melted and running, and concreted with it, without however ſuffering fuſion. Large broken maſſes of this *lava* are ſeen about the foot of the hill, very much reſembling fragments of ancient Roman buildings ruined; inſomuch, that I miſtook for ſuch the firſt maſs I ſaw, under a part of the hill, called Il Monte della Croce, from the church built upon it. The manner in which the Romans were accuſtomed to fill up the inner parts of their thick walls, within the facings, exactly reſembles this kind of Breccia *lava*; ſince they uſed irregular bits of ſtone, confuſedly thrown into, and cemented with a large body of mortar. This kind of *lava* is common enough about Veſuvius, and other recent volcanos; and alſo occupies large tracts in Upper Auvergne, eſpecially between Murat and Aurillac; where it covers the ſkirts of the hills, by the ſide of a valley, for many miles; forming the moſt groteſque figures imaginable. I have alſo obſerved it, in plenty, in the environs of Puy, in Velay. The famous St. Michael's church at Puy is built on the ſummit of a high, iſolated, and almoſt pointed rock of this *lava*, and makes a very extraordinary appearance. Monte Nuovo ſeems to have ſuffered a more recent conflagration than any other of the Euganean hills, as its name rather implies. But though its ſkirts and ſurface appear, almoſt every where, to have ſuffered a fuſion; yet parts of the internal ſtructure of the mountain, manifeſt, however, the primary horizontal direction and parallelifm of the *ſtrata*, as I before remarked of the Veroneſe vulcanic hills. This I particularly obſerved within the park of Cataio palace, which is ſituated at the north-eaſt extremity of Monte Nuovo, and has parts of its under apartments cut out of the ſolid rock of *lava*. Suppoſing Monte Nuovo iſolated from the low narrow iſthmus before mentioned, it would meaſure about ſix miles in circumference at the baſe. I ſhall further obſerve, that it forms a ſort of half moon, on the weſt ſide of the plain near Battaglia, and ſtands in the centre of ſeveral hot ſprings; as thoſe of St. Elena, and San Bartolomeo, to the ſouth and weſt; thoſe of Abano and Monte Ortone to the north, and many others. The neighbourhood of Monte Nuovo is alſo rendered intereſting to naturaliſts by its foſſil glaſs, *vitrum obſidianum* PLINII, *pumex vitreus ſolidus* LINNÆI, which is found in plenty under the weſt ſide of it, in a ſmall valley, called Val San Zibio, near the baths of San Bartolomeo. It exactly anſwers to the characters given of it by LINNÆUS and others; as may be ſeen by ſome ſpecimens of it, which I have ſent, and which were broken from a very large maſs. It abounds about the volcanos of the Andes particularly; but I found none of it in Auvergne, Velay, or the Veroneſe or Vicentine vulcanic hills. Monte Caſtello, or the Caſtle Hill, near Baon, at the ſouth-eaſt ſkirt of the Euganean hills, and about half a league from Eſte, affords another vulcanic concretion of a very remarkable ſtructure. This hill is moſtly formed of huge oval and laminated maſſes(o), of various ſizes, confuſedly concreted together, like a pudding ſtone, but in a vulcanic matrix, conſiſting of a ſort of dark-brown ſtone, with angular *lapilli* in it of a dingy-whitiſh colour, and viſibly manifeſting an affinity with the ordinary granite of the other neighbouring hills; and perfectly ſimilar to the ſigured maſſes concreted with it. In other parts, and particularly at Monte Galda, a ſmall iſolated elevation in the plain of Padua, between the Euganean and Vicentine hills, I have found theſe laminated maſſes of a ſpherical figure. In a broken cavity of a mixed vulcanic and marine hill of Monte Galda, I ſaw a group of theſe laminated round balls regularly placed one above the other, and

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(o) Fig. 4.

perpen-

Fig. 1

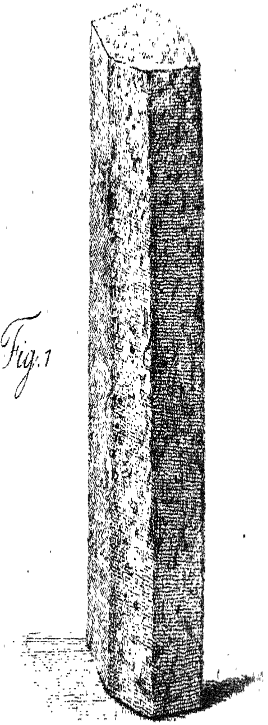


Fig. 2.

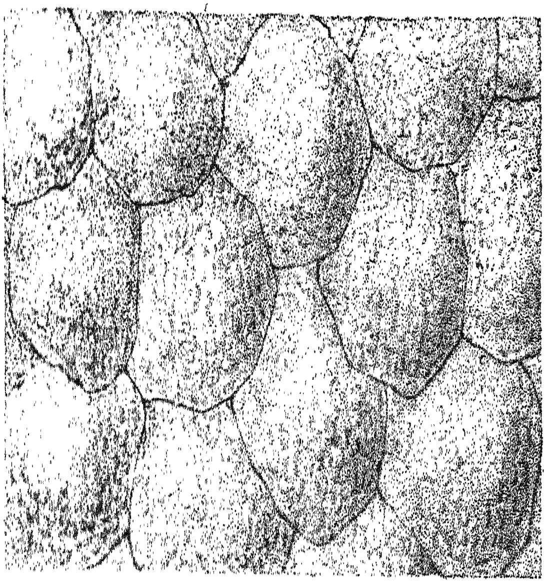


Fig. 3.

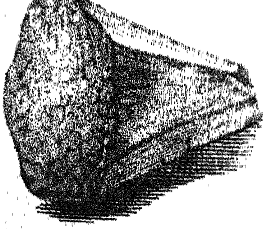
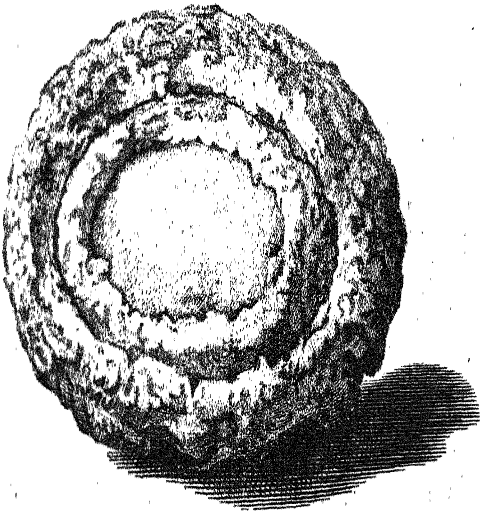


Fig. 4.



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perpendicularly, in columns, as it were. The annexed figure(p), though ſketched chiefly from memory, may ſerve to give ſome idea of this phænomenon. I have often obſerved the granite, of the Euganean hills particularly, affecting ſuch orbicular and laminated forms; as may be ſeen in the figure(q) repreſenting a perpendicular ſection of a ſimilar bank in theſe hills. Indeed, from this and other facts, which I have occaſionally mentioned in the courſe of my paper, there ſeems to be a ſtrong analogy between granites and many particular vulcanic concretions. Another of the Euganean hills, called Monte Uliveto, or the Mount of Olive Trees, near Teolo, is chiefly compoſed of a confuſed aggregate of ſmaller globular vulcanic balls, which are rather ſolid than laminated, and of a hard ferruginous ſubſtance, of a dark-brown colour, much reſembling ſome common ferruginous *geodes* I have ſeen. The annexed figure(r) repreſents a group of them. This may ſuffice for a ſpecimen of the more curious vulcanic productions of the Euganean hills, and united to the obſervations before made, may perhaps ſhow, how little we are acquainted with the ſtructure and relative phænomena of vulcanic bodies in general. I ſhall not enter into any account of other more common productions of this kind; and ſhall only mention, that, during my ſummer's reſidence at the baths of Abano, I made a collection of the Euganean *lava*, which is now in the public muſeum at Padua. Since ſuch productions are rather calculated to illuſtrate the phyſical topography of the coun-

(p) Fig. 5. (q) Fig. 6. (r) Fig. 7.

try to which they belong, than to adorn a cabinet, they muſt be more uſeful upon the ſpot, than they poſſibly can be elſewhere; which conſideration will, I hope, ſerve as my apology for not having tranſmitted them to the Society with the other ſpecimens. It ſeems rather extraordinary, that ſo curious a tract as that of the Euganean hills, which differs from all others in Italy, ſhould have remained ſo long unknown, eſpecially being in the neighbourhood of frequented baths, and a celebrated univerſity. For though BACCIUS, in his book *De Thermis*, ſuppoſes ſubterraneous fire, to account for the heat of the waters of Abano, and alſo mentions pumice ſtones about the baths there; yet he means only the porous and calcareous concretions formed by depoſition from thoſe waters; nor does he ſeem to have been at all acquainted with the indiſputable vulcanic phænomena, which the adjacent hills in plenty afford. Nor does VANDELLI enter into any obſervations of this kind, in his more modern and expreſs treatiſe *De Thermis Patavinis*; if we except the mention of the foſſil glaſs of Val San Zibio, which he ſeems to have firſt obſerved. I therefore hope, that the foregoing obſervations will be the more acceptable. It appears alſo from hence, if I miſtake not, that the Venetian territory in general abounds full as much with vulcanic phænomena, as any other part of Italy. For, beſides my obſervations in the Paduan, Veroneſe, and Vicentine territories, I have ſeen *lava*, in the public muſeum at Padua, from the Breſcian hills; and have obſerved *lava* pebbles in quantity in the beds of the rivers flowing from the mountains of Friuli. I therefore doubt not but theſe provinces alſo abound with vulcanic phænomena, though I have never had an opportunity to viſit them. I mention theſe circumſtances more willingly, ſince it has been generally imagined, that the northern parts of Italy contained few, if any, ſuch productions. They are, however, not only full as common, but, if I miſtake not, more inſtructive than thoſe of any other province of that country. For, beſides the phænomenon of Monte Roſſo, on the importance of which I have already inſiſted, and the other curious vulcanic productions of the Euganean hills, I muſt obſerve, that, from the inordinate courſe of the Appenines in general, the vulcanic hills of that chain afford no obſervation ſo intereſting to phyſical geography and the theory of ſuch phænomena, as that before remarked of the correſpondent direction and paralleliſm of the vulcanic and other branches of the Veroneſe and Vicentine diſtricts. My obſervations alſo, on the vulcanic branches of theſe diſtricts, do not ſeem to agree with the celebrated Monſ. GUETTARD's principle, who ſuppoſes(s), that all vulcanic materials obſervable in calcareous countries are adventitious; the contrary of this being indeed demonſtrated by the facts I have advanced. Nor have I entered particularly into an account of my vulcanic tour in the Venetian ſtate, that I might not abuſe myſelf of the ſufferance of ſo reſpectable a Society by an unintereſting detail of

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(s) Memoire ſur la Mineralogie d'Italie, in the firſt volume of his Memoires ſur les Sciences et les Arts.

facts, however juſtifiable I might have been, by the importance of ſome of them. For another imperfect baſaltine columnar group, of which I have alſo a drawing, exiſts near Gambellara, in the Veroneſe territory, a few miles from Montebello; and, according to the informations I have received, they are equally prevalent in the adjacent diſtrict of Vicenza. Doctor FESTARI, an ingenious phyſician of Valdagno, whoſe curioſity I have fortunately excited in theſe matters, lately informs me, that he has diſcovered a ſimilar group of priſmatic columns, in the mountains of that neighbourhood; and I had before been apprized of another near Maſon, not far from Baſſano, by Mr. ARDUINI, a celebrated naturaliſt of Venice. I have obſerved fragments of priſmatic columns about Maſon, but did not ſee the group. When I was at San Giovanni Illarione, I was alſo informed, that, at ſome diſtance, by the ſide of the torrent below, another ſimilar group exiſted. Nor have I the leaſt doubt of the frequency of ſuch phænomena, eſpecially in the Vicentine hills, where vulcanic effects are more common even than in the neighbouring territory of Verona. For of all the numerous lines, or branches, of mountains, that diverge from the chain of the Alps, and interſect, nearly in parallel directions, the Vicentine diſtrict, there is not one, I believe, but what contains more or leſs *lava*, and in quantity; whereas many of the ſaid branches, in the Veroneſe territory, are excluſively marine and calcareous; eſpecially in the immediate neighbourhood of Verona, about the Val Pantena, the Val Policella, and towards the Adige; and the Veroneſe mountains, that moſtly abound with *lava*, are thoſe of the eaſt and north-eaſt quarter, about the confines of the Vicentine territory, in the common road from Verona to Vicenza. I firſt perceived vulcanic effects in the environs of Caldiero, where the hot ſprings riſe. The immediate hills about them, which are iſolated in the plain, though of little elevation, are almoſt excluſively vulcanic; as are likewiſe the neighbouring points of the Alpine branches before mentioned. None of the writers on the baths of Caldiero take any notice of theſe facts, though they ſeem almoſt inſeparable from the conſideration of the origin and properties of thoſe waters. The ſame defect is obſervable alſo in the writers on the hot ſprings near Viterbo, which are in the centre of vulcanic hills, and on the other *Thermæ* near Radicofani, in the confining part of the Tuſcan ſtate, where, as I before obſerved, vulcanic effects alſo abound. This neglect indeed is but too common to other writers on *Thermæ* and mineral waters in general; phyſical topography ſeldom forming a part of their inquiries, however pertinent and even neceſſary in itſelf.

Having often had occaſion to ſpeak of Abano in the courſe of this paper, I cannot conclude it without mentioning an extraordinary phænomenon in the animal kingdom, which is obſervable there. Notwithſtanding the heat of thoſe waters, in which FAHRENHEIT's thermometer riſes to eighty-eight degrees, a particular ſpecies of *buccinum* breeds and lives in them, and is found in great plenty. It is of the fluviatile kind, and ſeems to be peculiar to theſe waters, having never ſeen nor heard of them in any others. They are remarkably ſmall, ſcarcely exceeding a line or two in length, and are perhaps the ſmalleſt univalve or teſtaceous animal of any ſuch kind hitherto diſcovered. It is mentioned and figured by VANDELLI in his treatiſe *De Thermis Patavinis*; but the figures are not good, and much too large; as may be ſeen by the original ſpecimens herewith ſent.

Such are the obſervations, which I have the honour to preſent to you, SIR, and to the other learned members of the Royal Society upon the preſent occaſion. I ſhall think myſelf very happy if they afford any ſatisfaction; and more particularly ſo, ſhould they be found conducive to the advancement of ſo intereſting a province of Science as that of Phyſical Geography, which being grounded upon facts, that require obſervation, ſeems hitherto to have ſuffered for the want of it. If, contrary to the common opinion, I have inſiſted on the local origin of moſt vulcanic tracts, it may further be conſidered, that this ſeems full as conſiſtent with the principle of their origin, as it is agreeable to the phænomena themſelves. For fire not only penetrates, pervades, deſtroys, and new modifies the texture of the moſt ſolid bodies; but is alſo often generated in theſe bodies, without the previous intervention of other fire; which conſideration alone might lead us to the opinion I have advanced, were there not ſuch evident proofs in ſupport of it. Theſe will, I hope, receive a ſtronger confirmation from a more particular account of the vulcanic phænomena of Auvergne and Velay,

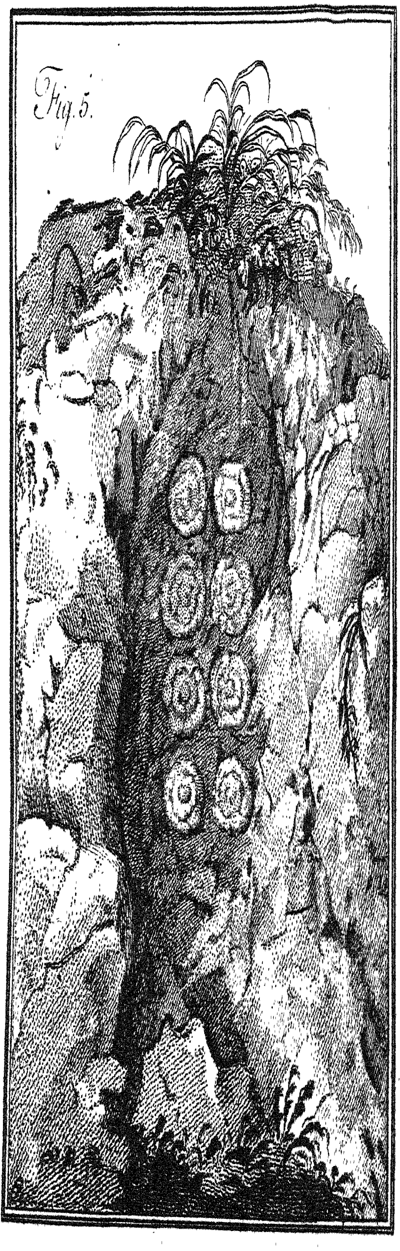
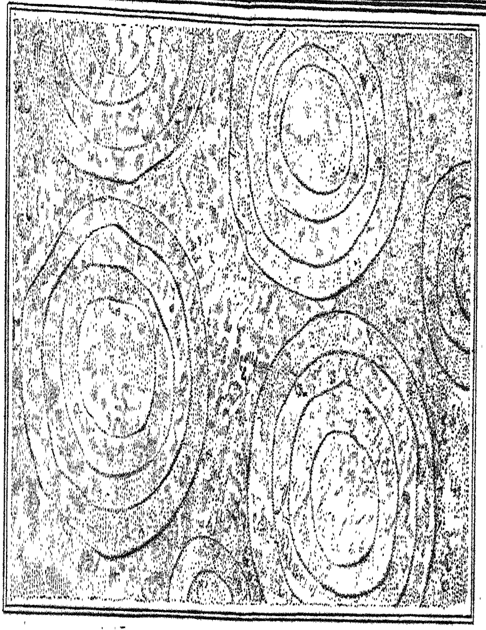


Fig. 5.

Fig 6



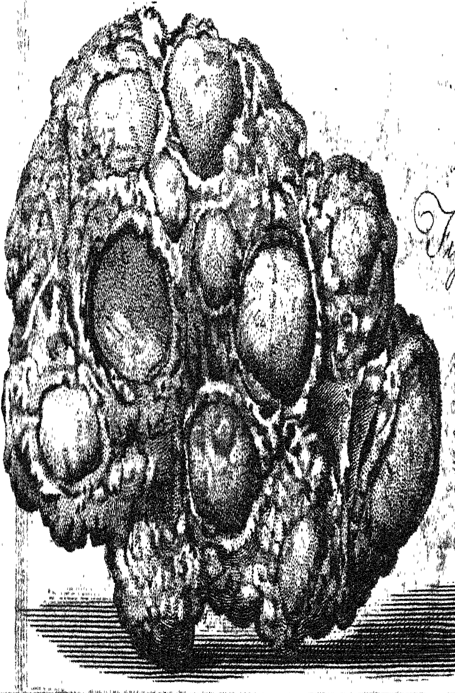


Fig 7

Basire sc.

Velay, which I propoſe myſelf the honour of preſenting to the Society upon a future occaſion. In the mean time I am, with great reſpect and eſteem, SIR,

Your moſt obedient

and very humble ſervant,

JOHN STRANGE.

P. S. Since the termination of the foregoing paper, I am informed by Mr. CHARLES HAY, of Brecknock, in South Wales, that the priſmatic baſaltine columns of Towen before mentioned were actually brought from Cader Idris, at the top of which mountain there are prodigious quantities of them, which are totally different from the rocks around them. This information Mr. HAY has lately received from a perſon informed; having obligingly made the inquiry at my requeſt. I may, perhaps, be enabled hereafter, through the ſame channel, to tranſmit to the Society a more particular account of the phænomenon; preſuming, that, in ſome part or other of Cader Idris, theſe columns form a regular group, as they commonly do in other places.

III. *An Inquiry to ſhow, what was the ancient* Engliſh *Weight and Meaſure according to the Laws or Statutes, prior to the Reign of* Henry *the Seventh.*

Redde, Nov. 24, 1774.

WILLIAM the Conqueror, by his charter, confirmed to the Engliſh all their ancient laws, with ſuch additions or alterations as he made therein, to their advantage. The 57th clauſe of that charter is, "*De menſuris et ponderibus. Et quod habeant* "*per univerſum regnum, menſuras fideliſſimas et ſignatas,* "*et pondera fideliſſima et ſignata ſicut boni prædeceſſores* "*ſtatuerunt.*" From this clauſe it ſeems clear, that king WILLIAM ordained, ſealed ſtandards both of weights and meaſures, to be made, ſuch as his predeceſſor king EDWARD had ordained. Neither weights or meaſures are here deſcribed particularly; but the ſubſequent ſtatutes define them more plainly. And the *Chronicon Pretioſum* tells us, that from hiſtorians it appears, the Conqueror determined what the weight of the ſterling penny, or penny weight, ſhould be, to weigh 32 grains dry wheat. Conſequently the ſtandard penny weight was made equal to the weight of 32 grains wheat. Succeeding kings confirmed WILLIAM's charter; and even the great charter granted by king JOHN, is only to explain and reſtore the ancient laws, which had been infringed. The ſtatutes of 51ſt of HENRY III. and 31ſt of EDWARD I. explain the ancient weights and meaſures; that is to ſay, the Engliſh penny called a ſterling, round without clipping, was to weigh 32 grains dry wheat, taken from midſt of the ear, and 20 of thoſe penny weights were to make an ounce, and 12 ounces a pound; and 8 of thoſe pounds were to be a gallon of wine, and 8 of thoſe gallons to make a London buſhel, which is the $\frac{1}{8}$th part of a quarter. The definition of the penny weight in theſe ſtatutes agrees with the determination of WILLIAM the Conqueror, and ſhows the legal weight continued the ſame. What the weight of that pound was, ſo raiſed from a penny weight, equal to the weight of 32 grains of wheat, we may clearly learn from that declaration in the 18th of HENRY VIII. when he aboliſhed that old pound, and eſtabliſhed the Troy weight; which ſays, that the Troy pound exceedeth the old Tower pound by $\frac{3}{4}$ of the ounce. As the Troy pound eſtabliſhed by HENRY VIII. is the ſame as is now in uſe, conſiſting of 5760 Troy grains, and 480 grains to the ounce, and 12 ounces to the pound: ſo 360 grains is $\frac{3}{4}$ of the ounce, which, deducted from 5760, leaves 5400 Troy grains, equal to the weight of that old Saxon pound which he aboliſhed. But to trace out experimentally the weight of that penny weight, raiſed from 32 grains of wheat, I got a ſmall ſample of dry wheat of laſt year 1773 (the wheat of that year but ordinary); and, from a little handful taken therefrom, I told out juſt 96 round plump grains, dividing them into

parcels

parcels of 32 grains each, and all three weighed exact 22½ Troy grains; conſequently, 240 ſuch penny weights, which the old pound conſiſted of, were equal only to 5400 of our preſent Troy grains, conformable to the declaration of HENRY VIII. Thus the weight of that old pound is clearly aſcertained to be lighter than the preſent Troy pound by ¾ of an ounce; and it clearly ſhews, they were two different weights. By thoſe ſtatutes of HENRY III. and EDWARD I. it is ſaid, that 8 pounds were to make a wine gallon, and 8 of thoſe gallons to be a buſhel, and 8 buſhels a quarter; conſequently the wine and corn gallon were one and the ſame meaſure. The ſtatute of the 12th of HENRY VII. ſays, the gallon meaſure was to be 8 pounds of wheat, which aſcertains what was to be underſtood by former ſtatutes, and is conſonant to reaſon, to fix the meaſure of wheat by its own weight, not by that of wine, as wheat was an article of greater importance to the community to aſcertain its meaſure than wine; and a gallon meaſure to contain 8 pounds of wheat, muſt be ¼ part larger in cubical contents than a meaſure to contain 8 pounds of wine. As it appears by the charter of WILLIAM the Conqueror, that there were ſealed ſtandards made of weights and meaſures, we cannot doubt, but they were preſerved and kept in the king's exchequer, for legal ſtandards; and as ſeveral ſtatutes direct their being made of metal, they were permanent and certain, whereby to make more: which HENRY VII. expreſsly tells us he practiſed, by making new according to the old: ſo that there could be no need to recur to 32 grains of wheat, much leſs to 7680, every time new ſtandards were to be made, unleſs we ſuppoſe our anceſtors defective in common ſenſe. Whenever, by new ſtatutes, freſh ſtandards were directed to be made, we may obſerve, the aſſize of weight and meaſure continued uniformly fixed and deſcribed to be one and the ſame, to ſhew there was no alteration made or intended. And thus, by the laws of Aſſize, from WILLIAM the Conqueror to the reign of HENRY VII. the legal pound weight continued a pound of 12 ounces, raiſed from 32 grains of wheat, and the legal gallon meaſure invariably to contain 8 of thoſe pounds of wheat, 8 gallons to make a buſhel, and 8 buſhels a quarter; the buſhel, therefore, contained 64 of thoſe pounds of wheat, and the quarter 512 pounds. Theſe were the legal weights and meaſures for common uſe, during that period. The firſt alteration, really made therein, was in the 12th year of HENRY VII. as will be mentioned hereafter. That the laws of Aſſize were often infringed, is very evident from the frequent complaints, mentioned in COTTON's Abridgement of the Tower Records, againſt the king's purveyors; particularly in the 14th of EDWARD III. for remedy againſt outrageous takings of purveyors; and in the 45th of EDWARD III. that the king ſhould be ſerved by *common meaſure*; and in the 3d of HENRY V. that the king's purveyors do *take* 8 *buſhels of corn only, to the quarter ſtriked*. The general anſwers whereto were, that the ſtatutes ſhould be obſerved. It appears alſo, that others infringed the laws of aſſize. For the ſtatute of 27th of

EDWARD III. ſays, Some merchants bought Avoirdupois merchandizes by one weight, and ſold by another; which plainly implies, they bought by ſome weight heavier than the legal, and ſold by the legal weight which was lighter; for it is rather too abſurd for ſuppoſition to imagine, they bought by a light weight, and ſold by a heavier. The ſtatute, therefore, to inforce obſervance of the laws of aſſize, only wills and eſtabliſhes, that there be, *one weight, one meaſure, and one yard, through all the land.* This can be underſtood to mean no other, than the legal aſſize, which preceding ſtatutes had enacted. And farther, in the reign of HENRY VI. we ſee that buyers of corn, bought by a veſſel, called a fatt, of 9 buſhels, which contained 72 gallons; and like thoſe merchants before mentioned in the ſtatute of EDWARD III. we may preſume they ſold by another meaſure, the legal quarter of 8 buſhels, containing but 64 gallons: for the ſtatute of 9th HENRY VI. forbids the buying by that veſſel, called a fatt. The prohibition implies the illegality of the veſſel and its uſe, and implies alſo the inforcement of the laws of aſſize. Taking therefore all the ſeveral ſtatutes together, in one connected view, thoſe that fix the laws of aſſize, with thoſe to reform abuſes committed againſt them, we are led to conclude, thoſe laws of aſſize continued uniformly one and the ſame, till HENRY VII. altered them. Having thus ſhewn by thoſe laws, that the old pound weight was a Saxon pound of twelve ounces, raiſed from 32 grains of wheat, and was equal only to 5400 of our preſent Troy grains; and that the meaſure of capacity was a gallon, a gallon, to contain 8 of thoſe pounds of wheat, and 8 of thoſe gallons made a buſhel: I ſhall now endeavour, by help of figures, to demonſtrate what was the cubical contents both of the gallon and buſhel meaſures.

We know, the preſent Troy pound conſiſts of 5760 Troy grains, and that 7000 of thoſe Troy grains are equal to the preſent Avoirdupois pound of 16 ounces, and that 5400 of thoſe Troy grains are equal to the old Saxon pound of 12 ounces; conſequently, the old Saxon pound was $\frac{540}{576}$ of the preſent Troy pound, and the old Saxon pound was $\frac{54}{70}$ of the preſent Avoirdupois pound. We know, modern experiment hath proved the weight of 1728 cubic inches of wheat, common ſort, to be $47\frac{1}{2}$ pounds Avoirdupois; and of a better ſort, to weigh from $48\frac{1}{4}$ to $48\frac{1}{8}$ pounds Avoirdupois, the difference in their weight is not very great; however, I will take the loweſt weight to compute by, the $47\frac{1}{2}$ pounds Avoirdupois, which, in Saxon weight, is $61\frac{31}{54}$ pounds Saxon. And then I ſay, as $61\frac{31}{54}$ pounds Saxon : 8 pounds Saxon :: 1728 cubic inches : $224\frac{1}{2}$ cubic inches, for contents of the old Saxon gallon for wine and wheat. But as the old ſtandard wine gallon, kept at Guildhall, and found there in 1688, proves to be 224 cubic inches contents, there is reaſon to conclude it to be of the ſame ſtandard aſſize, as was the ancient Saxon gallon for wine and wheat: for, as 1728 cubic inches : 224 cubic inches :: $61\frac{31}{54}$ pounds Saxon : $7\frac{53}{54}$ pounds Saxon, which is about $4\frac{1}{3}$ penny weights ſhort of the 8 pounds, mentioned in the ſtatutes for the gallon to contain, and is ſuch a ſmall difference, as may ariſe in different years, in the weight of ſuch a quantity of wheat. The very near agreement of theſe computations, gives us ſufficient reaſon to conclude, that the old ſtandard wine gallon, of 224 cubic inches contents, found at Guildhall in 1688, was of ſame ſtandard aſſize, as was the ancient gallon meaſure ordained to hold 8 Saxon pounds of wheat; and of courſe then, the buſhel meaſure muſt have been 1792 cubic inches contents, which will appear to hold nearly 64 Saxon pounds of wheat, as by thoſe old ſtatutes it ought to do. For, as 1728 cubic inches : 1792 cubic inches :: $61\frac{31}{54}$ pounds Saxon : $63\frac{1476}{1728}$ pounds Saxon, which is only about an ounce and three quarters ſhort of 64 pounds; and in ſo large a quantity of wheat, is a trifling difference, naturally ariſing in weight of wheat of different years. Theſe demonſtrations, by figures, ſufficiently prove, what the cubical contents of thoſe ancient Engliſh meaſures muſt have been, according to the old ſtatutes of aſſize; that is to ſay,

The gallon meaſure, 224 cubic inches contents, to hold 8 pounds Saxon.
The buſhel, 1792 ditto, 64 ditto.

And as 8 buſhels made a quarter, the quarter contained 512 Saxon pounds of wheat. Theſe were the ancient legal meaſures, according to the old laws of aſſize.

It now remains to mention the particular ſtatute of the 12th of HENRY VII. under which, an alteration was brought about in thoſe ancient weights and meaſures, without ſeeming to intend it; as the ſtatute itſelf differs not in ſubſtance from the other old laws of aſſize, except calling the pound by a new name, Troy. But previous thereto, it may be neceſſary to obſerve, that very probably, the unſettled ſtate of the kingdom for many years preceding, might pave a way to that alteration. There had been ſeveral conteſts about the crown, between the two houſes of York and Lancaſter, till HENRY VII. by conqueſt, mounted the throne; and in ſuch times of public diſturbance, the laws of aſſize were more likely to be infringed, than well kept. For, after HENRY VII. was well ſettled on his throne, we find complaint was made in the 11th year of his reign, that the laws of aſſize had not been obſerved and kept. Whereupon he made freſh ſtandards of weights and meaſures, and ſent them to the ſeveral ſhires and Towns in the kingdom. But in the very next year (the 12th of his reign) there came out that particular ſtatute, under which, the weights and meaſures were altered. Reciting, that the king, in the former year, had made *weights and meaſures of braſs*, according to the *old ſtandards thereof*, *remaining in his treaſury*, which weights and meaſures are ſaid, on a more diligent examination, to have been *approved defective*. It is not ſaid, whether they were the old ſtandard weights and meaſures, or the new ones, made in the former year, that had been approved defective; nor how much they were ſo: all this is left to conjecture. Therefore we may, with great probability, conjecture, they were not defective in reſpect to their old original ſtandard; but only in reſpect to the heavier new Troy pound, intended to be then introduced. And what warrants ſuch conjecture is, the expreſs declaration of his ſon HENRY VIII. when he aboliſhed the old pound, in the 18th of his reign, and eſtabliſhed the Troy; for he then declares, the Troy pound exceedeth the old pound by $\frac{3}{4}$ of an ounce. This ſets the matter in a clear light, and ſhews what the two weights were, and what the difference between them. Hence then, there can be no doubt, but HENRY VII. altered the old Engliſh weight, and introduced a heavier Troy pound, that exceeded the old one by $\frac{3}{4}$ of an ounce; and although none of his ſtandard weights have come down to us, yet his braſs buſhel meaſure, with his name upon it, was found in the Exchequer in 1688, and proves to be 2145 cubic inches contents; from which we may form concluſions, both on his weights and meaſures, ſufficient to convince us, that he altered both. That his buſhel was a meaſure of 9 gallons inſtead of 8, and that his Troy pound was $\frac{1}{16}$ part heavier than the old Engliſh pound, which was raiſed from 32 grains of wheat. Experiment hath proved, that a meaſure of 1728 cubic inches of wheat, will weigh from $47\frac{1}{2}$ to about $48\frac{1}{4}$ pounds Avoirdupois; but ſuppoſe it be only $47\frac{3}{4}$ pounds Avoirdupois, that, in Troy weight, will be $58\frac{17}{576}$ pounds Troy. From hence we may eaſily find the weight of wheat that 2145 cubic inches will contain. For, as 1728 cubic inches : 2145 cubic inches :: $58\frac{17}{576}$ pounds Troy : 72 pounds Troy, the weight of wheat that HENRY VIIth's buſhel would contain. And dividing the 72 by 8, the number of pounds limited by the ſtatute to a gallon, it proves HENRY VIIth's buſhel was a meaſure of 9 gallons inſtead of 8; and as 8 buſhels made a quarter, then the quarter contained 72 gallons.

gallons; this ſeems to correſpond with the number of gallons contained in the veſſel, called a fatt, the uſe of which was prohibited by ſtatute in HENRY VIth's time, about 60 years before HENRY VII. as hath herein been already remarked. If we divide the 2145 cubic inches contents of the buſhel, by 9, the number of gallons it contained, it ſhews the gallon meaſure to be $238\frac{1}{3}$ cubic inches contents, which is $\frac{1}{16}$ part larger than the old Saxon gallon of 224 cubic inches, juſt in the proportion as the Troy pound is $\frac{1}{16}$ part heavier than the old Saxon pound. The ſtatute limits the gallon to hold 8 pounds Troy of wheat; and ſo we find the gallon of $238\frac{1}{3}$ cubic inches will do: for as 2145 cubic inches : $238\frac{1}{3}$ cubic inches :: 72 pounds Troy : 8 pounds Troy. But if it be ſaid, the ſtatute limits the buſhel to 8 gallons, not 9, then the gallon meaſure muſt have been $268\frac{1}{8}$ cubic inches contents, and would hold 9 pounds Troy of wheat, though the ſtatute ſays it was to hold but 8 pounds Troy. Take it either way, it ſhews that the buſhel was not made according to the ſtatute; it held 72 pounds inſtead of 64 pounds. And upon the whole it clearly proves, that HENRY VII. altered both the weights and the meaſures; that he introduced the Troy pound, which was heavier by $\frac{3}{4}$ of an ounce than the Saxon or old Engliſh pound; and that his buſhel meaſure was about $\frac{1}{6}$th part larger than the ancient Saxon or old Engliſh buſhel meaſure. The firſt ſtatute that directs the uſe of the Avoirdupois weight is, that of the 24th of HENRY VIII. which plainly implies it was no legal weight, till that ſtatute gave it a legal ſanction; and the particular uſe to which the ſaid weight is there directed, is ſimply for weighing butchers meat in the market. And it is note-worthy, that in all the old ſtatutes of aſſize prior to HENRY VII. the legal gallon meaſure of capacity is founded on 8 pounds, raiſed from the weight of 32 grains of wheat, and by that ſtatute of 12th HENRY VII. the gallon is to contain 8 pounds Troy: therefore, theſe two ſorts of weight were the only ones eſtabliſhed as legal by the ſtatutes; and both are a lighter weight than Avoirdupois. How, or when, the Avoirdupois weight came firſt into private uſe is not clearly known to us; but this ſeems clear, that no ſtatute before the 24th HENRY VIII. hath given it any legal ſanction.

IV. *The Deſcription of an Apparatus for impregnating Water with fixed Air; and of the Manner of conducting that Proceſs. By* John Mervin Nooth, *M. D. F. R. S.*

Redde, Dec. 15, 1774. THE poſſibility of impregnating water with fixed air was no ſooner aſcertained, by experiment, than various methods were contrived to effect the impregnation. The ingenious Dr. PRIESTLEY, however, is the only one that has publiſhed any deſcription of an apparatus, calculated intirely for this purpoſe. This apparatus was communicated to the public, with the view of promoting the diſcovery of the medical effects of fixed air united with water; and, in conſequence of this communication, ſome very ſucceſsful attempts have been made in the cure of diſeaſes. The experiments, however, have not been ſo numerous as one could have wiſhed; perhaps the difficulty in conducting the proceſs, in the manner propoſed, has been, in ſome meaſure, the reaſon why ſo few experiments, on this ſubject, have been made public. For although, in the hands of the doctor, the apparatus was ſufficiently convenient, it muſt be confeſſed, that the conduct of the proceſs required more addreſs than generally falls to the ſhare of thoſe that are unaccuſtomed to ſuch experiments. Independent too of the inconveniences attending the proceſs, there was another objection to the apparatus, which, with moſt people, might have conſiderable weight. The bladder, which formed part of it, was thought to render the water offenſive; and when the ſolvent power of fixed air is conſidered, it will not appear improbable, that the water would be always more or leſs tainted by the bladder. In ſome trials which I made with Dr. PRIESTLEY'S apparatus, it always happened, that the water acquired an urinous flavour; and this taſte in the water was, in general, ſo predominant that it could not be ſwallowed, without ſome degree of reluctance. The difficulty, therefore, in the conduct of the proceſs, and the offenſiveneſs of part of the apparatus, made ſome leſs exceptionable method of producing the impregnation deſirable. This I variouſly attempted, keeping convenience and cleanlineſs conſtantly in view; and I flatter myſelf, that I have at laſt contrived an apparatus that will perfectly anſwer the intended purpoſe. It is now twelve months ſince this contrivance has been in conſtant uſe; and hitherto there is no reaſon to wiſh for the leaſt alteration. Preſuming, therefore, on the poſſibility of its becoming, when known, extenſively uſeful, and convinced of the favourable reception which every attempt of this nature meets with from the Royal Society, I beg leave to communicate to them a deſcription of the apparatus that I have invented, and of the manner of conducting the proceſs.

DESCRIPTION OF THE APPARATUS;

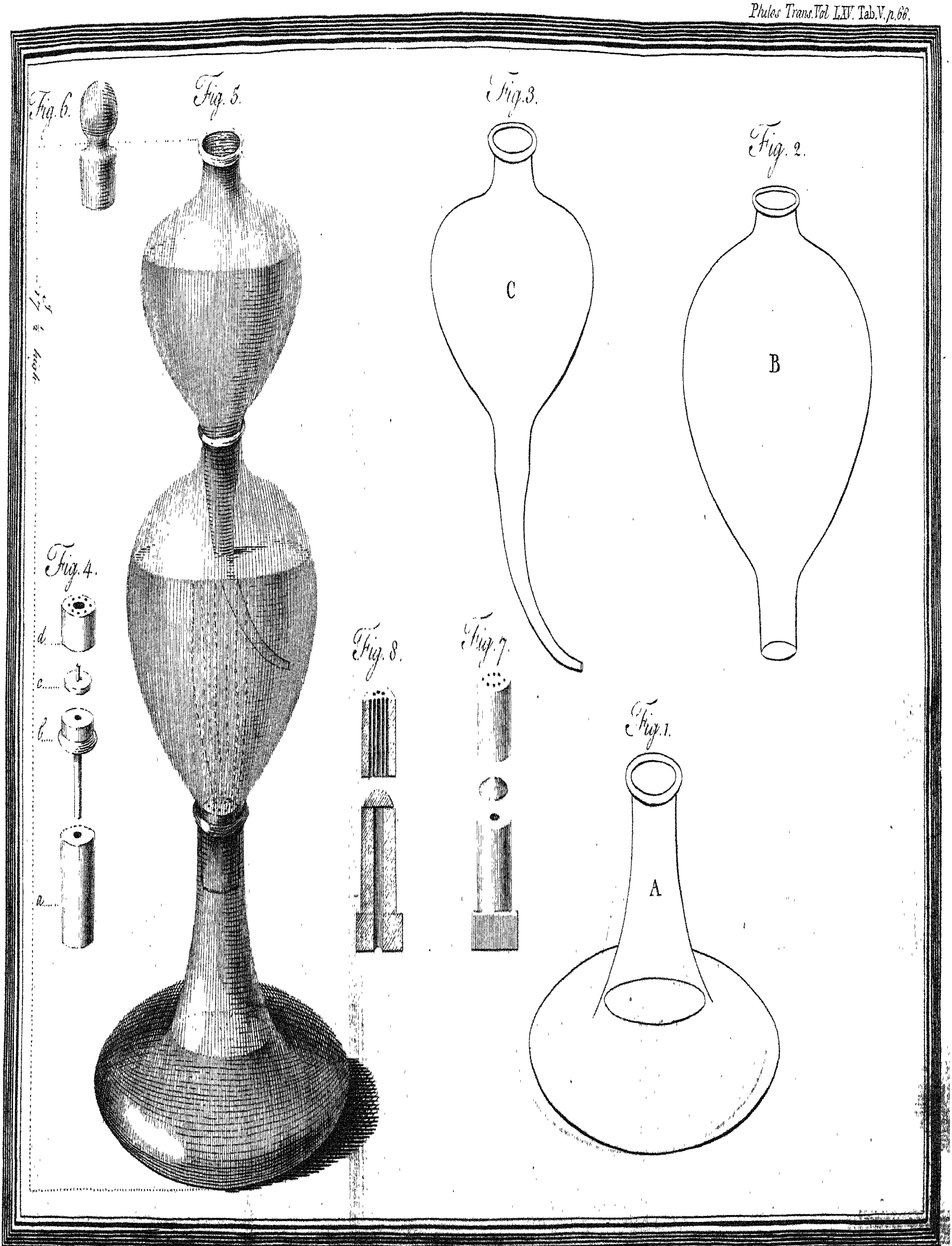
which is of glaſs, and conſiſts of three veſſels as (A, B, C), fig. 1. 2. 3. The glaſſes are accurately fitted to each other, and at the joints are impervious both to air and water. The glaſs (A) is deſigned for the efferveſcing ſubſtances. The veſſel (B) is to contain the water that is to be impregnated with air. In the lower part of the glaſs (B) is placed an ivory valve, ſurrounded with cork, as in fig. 4. The cork *(a)* is fitted to the bottom of the glaſs (B), and has through it an hole, to receive the part *(b)* of the ivory valve. On the broader part of this piece *(b)* is placed a moveable piece *(c)*. The ſurfaces of theſe pieces are ſo accurately ground, that, when applied to each other, no fluid whatever can paſs between them. The moveable part *(c)* is ſecured on the part *(b)* by the cover *(d)*, which is ſo conſtructed, as to allow the piece *(c)* ſome motion; and this cover has likewiſe holes to give paſſage to the air that ſhall raiſe the moveable piece *(c)*. The glaſs (C) ſerves two purpoſes; it confines the air on the ſurface of the water in (B), and at the ſame time prevents all danger of exploſion by allowing the water to give place to the aſcending air.

THE PROCESS.

As chalk and oil of vitriol are capable of producing the deſired efferveſcence, and are the moſt eligible on account of their cheapneſs, I ſhall, in deſcribing the proceſs, mention only theſe two ingredients. Variety of other ſubſtances may, however, be employed for the ſame purpoſe; but none, perhaps, are ſo unexceptionable as thoſe I have named. In the other acids a proper degree of fixity is wanting, during the efferveſcence; the nitrous and marine have ſo much volatility that there is always a riſk of ſome of the acid fumes paſſing the valve, and thus rendering the water acid, which it was intended to impregnate only with fixed air. To begin the proceſs, it is neceſſary to fill the veſſel (A) up to the dotted lines, with diluted oil of vitriol. By confining the height of the ſurface of the efferveſcing mixture to the dotted lines in the glaſs (A), none of the acid will be driven through the valve, during the intumeſcence that attends the eſcape of the fixed air. The glaſs (B) is to be totally filled with water, and the veſſel (C) is to be put on it. Some powdered chalk is then to be thrown into the glaſs (A), and the veſſels are to be immediately placed as in fig. 5. except that the ſtopper belonging to (C) is to be left out. When the acid, in the lowermoſt veſſel, acts on the chalk, the extricated air paſſes the valve in the middle glaſs; and as the conſtruction of this valve allows the fixed air from the efferveſcing ſubſtances to paſs, but denies a paſſage to the water in a contrary direction, the ſeparated air aſcends to the upper part of the middle glaſs, and at the ſame time a portion of water, equal in bulk to the intruding air, paſſes up the bent tube into the uppermoſt veſſel. As the efferveſcence goes on, the fixed air continues to accumulate in the middle veſſel, and the uppermoſt one to be filled with the water that has given place to the air. The quantity of chalk to be thrown into the acid at one time, muſt be determined by the capacity of the uppermoſt veſſel. Should more air be extricated than is ſufficient, in the conduct of the proceſs, to fill that veſſel, the water will run over the top of it, and will continue to run as long as any air aſcends in the middle veſſel, or till the ſurface of the water is below the extremity of the bent tube. Both theſe accidents are to be carefully avoided; as in one caſe, the whole would be wet and diſagreeable; and in the other, a quantity of fixed air would be unneceſſarily loſt. Half a dram of chalk will, in general, produce air enough to fill the uppermoſt veſſel with water; and it muſt be remembered, that the chalk employed to produce the effervеſcence, ſhould be finely powdered, as a ſelenitic cruſt will otherwiſe form around it, and thus prevent the action of the acid on the interior part. To keep the neck of the glaſs clean, through which the chalk is put, it will be neceſſary to include the chalk looſely in paper; and this circumſtance is by no means to be neglected, as the accurate junction of the glaſſes depends on it, and conſequently the whole of the proceſs. When the uppermoſt veſſel is filled with water, and there is, therefore, a conſiderable quantity of fixed air in the middle one, theſe two veſſels are to be ſeparated from the lowermoſt, and the air and water are to be agitated together, to promote their union. If, during the agitation, a ſtopper be put into the uppermoſt glaſs, the deſcent of the water in it will not ſhew the abſorption of the fixed air by the water, as the external atmoſpherical air will enter below, at the valve, to fill the ſpace which the abſorbed fixed air would otherwiſe leave void. But, on the contrary, if the uppermoſt veſſel be open, during the agitation, the preſſure of the atmoſphere on the ſurface of the water in that veſſel, will force the water down into the middle one, as faſt as the abſorption of the fixed air below will allow it room. This latter method may be purſued, when a perſon wiſhes to know the quantity of fixed air that the water can abſorb; but in common uſe, it will be better to ſtop the uppermoſt veſſel, as the air and water may be then more forcibly agitated without inconvenience, and of courſe, the impregnation more expeditiouſly effected. During the efferveſcence, the uppermoſt glaſs is to remain open, and it is only to be ſtopped when the agitation is performed. It is not to be expected, that the impregnation will be conſiderable at firſt; it will indeed be neceſſary to repeat the proceſs, with the ſame water, four or five times, before it will be highly impregnated. After an agitation, therefore, when a ſtronger impregnation is wiſhed for, the uppermoſt veſſel is to be opened, and raiſed from the middle one, to allow the water to deſcend, that was before driven up. When the middle glaſs is again full, a freſh quantity of chalk is to be put into the lowermoſt veſſel, and the agitation to be repeated, as ſoon as the efferveſcence ceaſes. It is ſeldom neceſſary to repeat the proceſs more than four times, to produce a very ſtrong impregnation; but ſhould it be thought proper, to have the water as highly ſaturated with fixed air as it admits of, nothing more than a repetition of the ſame proceſs is requiſite. In this account of the apparatus, I have purpoſely confined myſelf to the method of uniting fixed air with water; but it is to be obſerved, that many curious experiments may be made with it, both in chemiſtry and pharmacy. By its aſſiſtance, I have been enabled to imitate very perfectly, the common mineral waters, and to make aqueous ſolutions of ſubſtances that were before deemed inſoluble in water. Theſe circumſtances, however, I ſhall reſerve for a future paper, which I ſhall have the honour to preſent to the Society, as I have not yet been able to arrange the ſeveral facts, which this apparatus has made me acquainted with, in the manner I could wiſh.

POSTSCRIPT.

SINCE the foregoing paper was read, I have contrived a glaſs valve, which ſeems preferable in ſome reſpects to the ivory one therein deſcribed. The following is a deſcription of it. It conſiſts of three pieces, as in fig. 7. The ſuperior and inferior pieces are perforated, but the middle one is without perforation, having only its upper part convex and its under part plane. In fig. 8. is a perpendicular ſection of the three pieces compoſing the valve, at the diſtance at which they ought to be placed, with reſpect to each other, in the tabular part of the veſſel (B). This veſſel having the glaſs valve in it, and filled with water, is to be put on the glaſs (A), containing ſubſtances in the act of effervеſcence. In that caſe, the extricated air will aſcend through the perforations in the ſuperior and inferior pieces, the middle one proving no obſtacle to the air, having ſufficient room to yield to the current of air ruſhing upwards; but when the air ceaſes to aſcend, and the preſſure of the water above takes place, the middle piece will prevent the water from deſcending, its plane ſurface being then applied to the plane ſurface of the piece below it. Thus, SIR, this glaſs valve will anſwer in every caſe where the ivory one can be employed; and for a variety of purpoſes it will undoubtedly prove preferable, particularly when corroſive ſubſtances are ſubjected to experiment.



Basire sc.

V. *Account of a Muſical Inſtrument, which was brought by Captain* Fourneaux *from the Iſle of* Amſterdam *in the* South Seas *to* London *in the Year* 1774, *and given to the Royal Society. By* Joſhua Steele, *Eſquire, in a Letter to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Margaret-Street, Cavendiſh-ſquare,
December 1, 1774.

SIR,

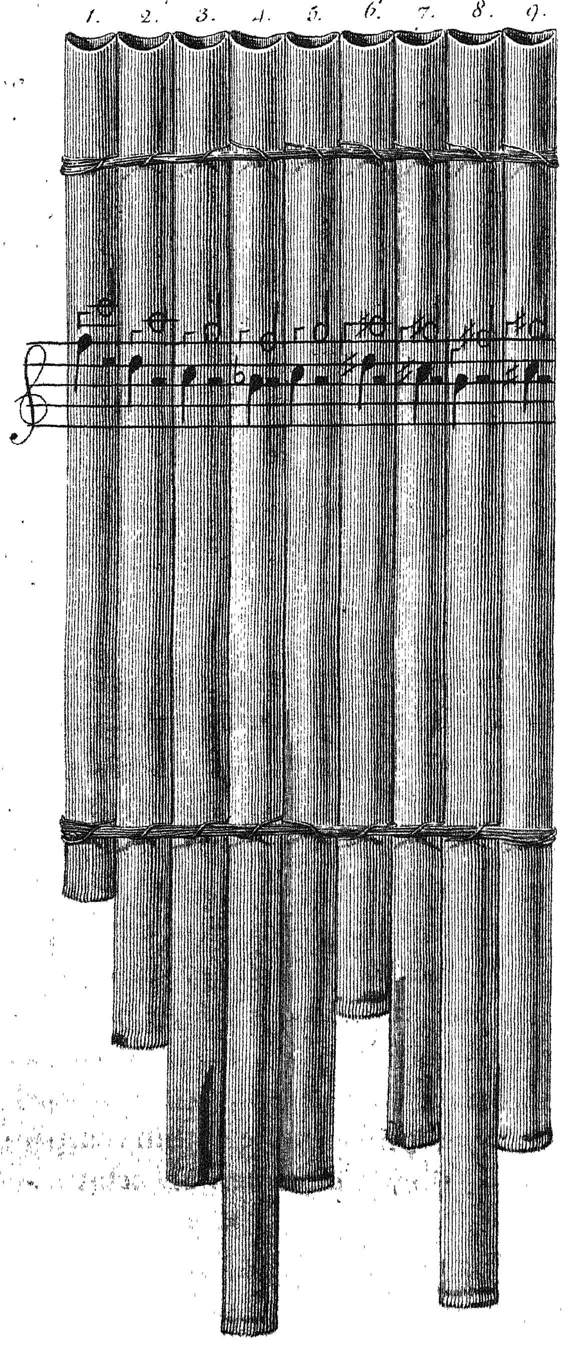
Redde, Jan. 22, 1775.

AGREEABLE to your requeſt, I have examined the curious ſyſtem of pipes, brought by Captain Fourneaux from the South Seas. The reſult of my experiments are herewith incloſed. The inſtrument was ſo new to me, that I ſhould be ſorry its reputation ſhould reſt intirely on my report, as I think an expert blower of the German flute might make further diſcoveries; towards which, my obſervations, whether perfectly accurate or not, may in ſome meaſure ſerve as a guide. The accident of a flat third, coming in the ſtead of a ſharp one, from the pipes 6. 7. 8. and 9. is ſo extraordinary, that I ſuſpected, for ſome time, the loweſt (or fundamental) tones of thoſe pipes were a quarter tone (or *dieſis)* lower than I have marked them; but, after repeated trials, and by the beſt judgement I could form by my ear, and by compariſon with another inſtrument, I gave up that ſuſpicion; and being confirmed in the opinion, that the moſt acute tones I could obtain from thoſe four pipes, were *minor thirds* to the moſt grave, I have ventured to mark them ſo. The reaſon why there was room for my doubt above mentioned is, becauſe the difference of hotter or colder, moiſter or dryer, has a ſenſible effect on the acuteneſs or gravity of the tones.

I am, SIR, with
great regard,
Your moſt humble ſervant,

JOSHUA STEELE.

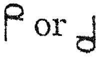
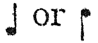
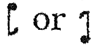
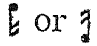
Figure of the System of Musical pipes, accord.g to their exact size

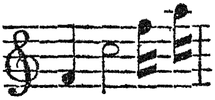


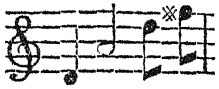
Explanation of the ſyſtem of muſical pipes, brought from the Iſle of Amſterdam in the South Sea, by Captain Fourneaux, to London, *anno* 1774, from experiments made by Mr. STEELE.

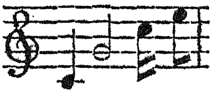
The manner of blowing theſe pipes, in making theſe experiments, was the ſame as people uſe to whiſtle in the pipe hole of a drawer key. Of the tones, marked on the drawing, the upper ſeries, which are exact fifths to the lower, are eaſieſt produced by an unexperienced perſon; and the lower ſeries, which we will call fundamentals, with ſomewhat more addreſs and a weaker blaſt. Beſide the above mentioned tones, if the velocity of the breath be increaſed a little, the five firſt pipes will give octaves to the fundamentals; and if farther increaſed, ſharp thirds, or tierces, above theſe octaves. In the pipes 6. 7. 8. and 9. I could neither make the octaves to the fundamentals, nor the ſharp tierces; but in their ſtead, the minor, or flat-third, above the octave came, when the breath was urged beyond the degree requiſite to produce the fifth. This minor third is an accident out of the natural order of tones produced from ſimple tubes, which I do not pretend to account for. Here following, are ſet down the notes of the ſeveral tones which I produced from each pipe; but, in order to bring them more within compaſs of the ſcale of five lines, they are written an octave lower than they

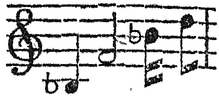
really

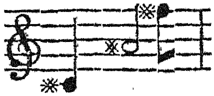
really are on the pipes. And alſo thoſe tones which come with moſt eaſe are wrote in minims, as 
thoſe in the next degree, in crotchets, as 
thoſe ſtill more difficult, in quavers, as 
and the moſt difficult in ſemiquavers, as 

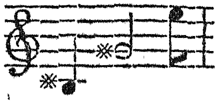
1ſt pipe 

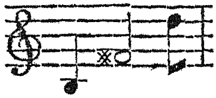
2d pipe 

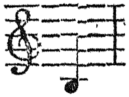
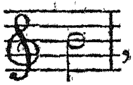
3d and 5th pipes uniſons 

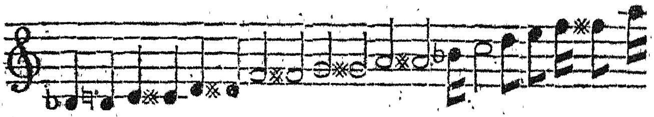
4th pipe 

6th pipe 

7th and 9th pipes uniſons 

8th pipe 

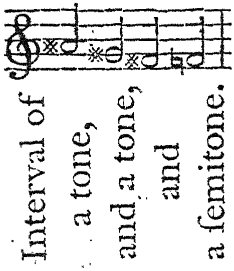
Theſe tones are adapted to Engliſh *conſort pitch*, by the above notes. From whence it is evident, that an expert performer may exhibit the following ſeries, and perhaps alſo the octave to the fundamental ; *videlicet*, , though I could not, which ſeries is ſufficient for an infinite number of airs:

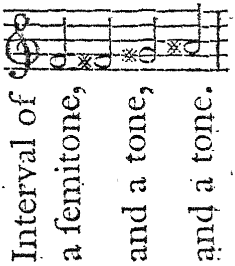


In this ſeries the notes marked in minims, being thoſe which are eaſieſt to be ſounded, furniſh two ſyſtems which correſpond with the definitions of the diatonic

and

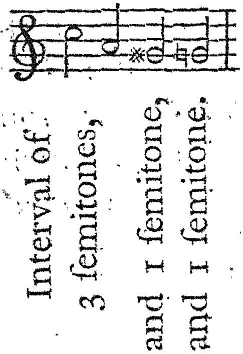
and chromatic *genera*, according to Euclid, who deſcribes the diatonic in deſcending, κατὰ τόνον, ϗ τόνον, ϗ ἡμιτόνιον; and in aſcending, καθ' ἡμιτόνιον, ϗ τόνον ϗ τόνον.

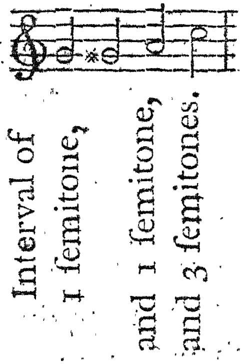
as 

deſcending, and 

aſcending.

And the chromatic thus, κατὰ τριημιτόνιον, ϗ ἡμιτόνιον, ϗ ἡμιτόνιον, in deſcending; and aſcending, καθ' ἡμιτόνιον, ϗ ἡμιτόνιον, ϗ τριημιτόνιον.

as 

deſcending, and 

aſcending.

But as the enharmonic *genus* requires intervals of the *dieſis*, or quarter tone, and as it did not appear by theſe experiments, that the pipes could exhibit any ſounds by ſuch intervals, I conclude they are not capable of performing according to the enharmonic diviſion of the tetrachord.

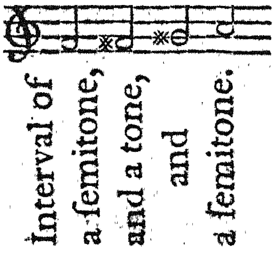
VI. *Remarks on a larger Syſtem of Reed Pipes from the Iſle of* Amſterdam, *with ſome Obſervations on the Noſe Flute of* Otaheite. *By* Joſhua Steele, *Eſquire.*

TO SIR JOHN PRINGLE, BART. P. R. S.

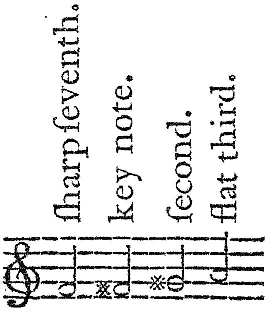
Margaret-ſtreet,
Feb. 21, 1775.

SIR,

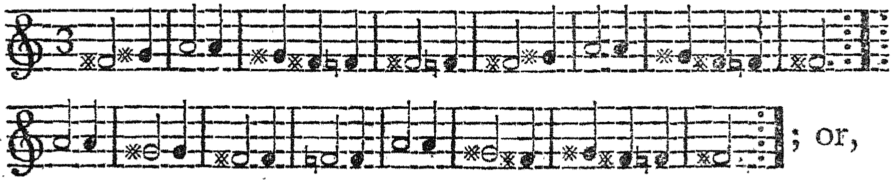
Redde, Feb. 22, 1775. THE notice taken of my ſmall endeavours, by your illuſtrious Society, does me much more honour than I deſerve; however, I receive it, as I ought, with reſpect and gratitude. I now incloſe to you ſuch farther remarks as I have been able to make, by repeated trials, on the laſt reed pipes you brought me from Mr. BANKS; which, though much larger, and more in number, are of the ſame *genus* with the former. I have alſo examined the noſe-flute of Otaheite, which Mr. BANKS favoured me with; and I find it gives only four ſounds, with the firſt degree of breath, which are, in an aſcending ſeries, by a ſemitone, a tone, and a ſemitone. Thus noted in conſort-pitch,



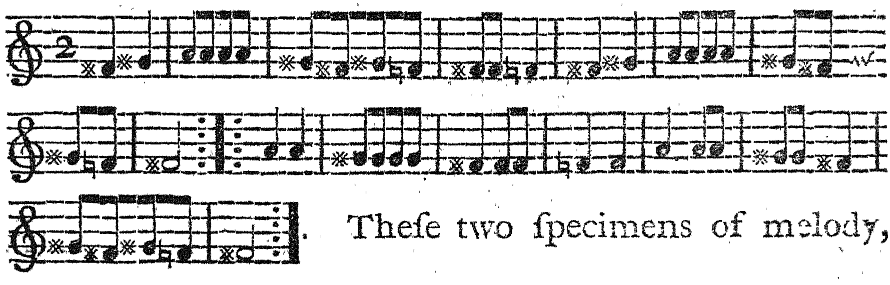
If urged with a ſtronger breath, it will give octaves above theſe; but it then becomes ill in tune: and I underſtood from Mr. Banks, the natives of Otaheite uſe no more than thoſe firſt four ſounds. Were I to give theſe notes denominations according to our ſyſtem of muſic, they ſhould be diſtinguiſhed thus,



Notwithſtanding the ſmall extent of this ſeries, yet, by the aid of varying the meaſure, it is capable of ſeveral different melodies, though the general caſt of them will be melancholy. As for example,



; or,



These two ſpecimens of melody, adapted to the noſe-flute, are, harmonically, the ſame, though rhythmically different; the latter having a degree of vivacity more than the former, in proportion to its meaſure of time; two bars of the firſt, being equal, in length, to three of the ſecond.

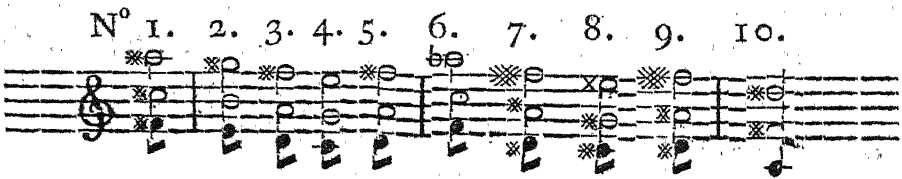
I am, SIR, with great regard,

Your very humble ſervant,

JOSHUA STEELE.

Remarks on the larger ſyſtem of reed pipes from the iſle of Amſterdam.

The ſpecific difference between this and the ſmaller ſyſtem, deſcribed before, will be underſtood from the following obſervations. It conſiſts of ten pipes, joined together in the ſame manner as thoſe of the ſmaller ſyſtem. The firſt nine pipes exhibit to the eye the ſame figure as the ſyſtem before deſcribed in the drawing; and the tenth pipe (which is the additional) is a little longer than N° 4. For in this larger ſyſtem, N° 8. is thirteen inches long; N° 4. thirteen and a half, nearly; and N° 10. is fourteen inches. The ſounds which each pipe *exhibits eaſily*, are marked in *minims*, as follows, and are noted agreeable to conſort pitch:

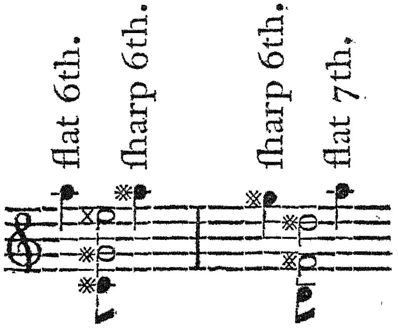


As the upper minims are ſixths to thoſe next under them, it follows, from the law of harmonic ſounds, that the lower minims are fifths to the fundamental ſounds of theſe pipes, which are written in quavers, to ſhew that they are very difficult to be produced. The upper minims of N° 1. 2. 3. 4. 5. and alſo of 10. are ſharp thirds, or rather, major tenths, to the fundamental ſound of each pipe. And the upper minims of N° 6. 7. 8. and 9. are nearly minor tenths to their fundamentals; which circumſtance ſeems to agree with what I remarked in the ſmaller ſyſtem, as an extraordinary property, touching the minor thirds. † But I will not yet aſſert, that this property is altogether natural, becauſe I found ſome of theſe latter pipes were partly obſtructed by accidental rubbiſh, which was drawn out with difficulty; ſo that I pretend not to decide, whether the cauſe of their being, not quite, in the ſame proportion of tune, as I found in the firſt ſyſtem, ariſes from ſome caſual injury, or from original intention, or original inaccuracy. ‡ I have ſaid, the upper minims of N° 6. 7. 8. and 9. are *nearly* minor tenths to their fundamentals; becauſe, in fact, I found them ſomething more than *minor*, and yet not *major*; wherefore I have uſed the mark (⋇), of a triple croſs, to ſignify ſomething more than (✱), the double croſs; and the mark of (×), a ſingle croſs, to ſignify a *dieſis*, or ſomething leſs than (✱), the double croſs; which laſt, in the modern practice of muſic, always means to ſay, *plus a ſemitone*, neither more or leſs. For though

† and ‡ referred to from the following page.

the

the nicety of the *dieſis* is ſtealing inſenſibly into the fancy of ſingers, and of ſome other elegant muſical performers, it is not as yet adopted, or uſed as ſuch, in the notation of modern muſic. The interval between N° 1. and 2. in theſe pipes, is only of two ſemitones; whereas, that between the N° 1. and 2. of the former ſyſtem, was of three ſemitones. The ſeries N° 2. 3. 4. and 5. and the ſeries N° 6. 7. 8. and 9. (both of which I have diſtinctly marked within bars) have ſimilar intervals in both ſyſtems (making allowance for what I have ſaid in page 75, † and ‡.) Wherefore I imagine theſe to have been the original extent of the whole modulating ſeries, like the double tetrachord of the Greeks, and that the N° 1. and N° 10. are additionals at pleaſure; as, in the ſmaller ſyſtem, the interval between N° 1. and 2. was a ſemitone greater than that between N° 1. and 2. in the larger ſyſtem; and N° 10. in the ſmaller ſyſtem (firſt examined) was totally omitted, though I have ſeen two others which had it. The ſounds in this larger ſyſtem are ſeven tones lower than thoſe of the ſmaller, which correſponds with the difference of their dimenſions; the pipe N° 4. in this ſyſtem meaſuring nearly thirteen inches and a half in length, with diameter ſeemingly proportional; whereas the N° 4. in the ſmaller ſyſtem meaſured only ſeven inches and a quarter. By increaſing the velocity of the blaſt, I found theſe pipes gave ſounds ſtill higher, which were *fourths* above the upper minims, or *octave and ſixths* above the fundamentals; and with a little more force, *tritones*, or *ſharp fourths*, above the upper minims, which were *octave and flat ſe-venths* above the fundamentals. But theſe two (the 4th and ſharp 4th above the upper minims) ſhould rather be conſidered as one *note of latitude*, which by more or leſs velocity, or force of breath, makes in the N° 1. 2. 3. 4. 5. and 10. either a ſharp 6th, or a flat 7th, to each of the fundamentals; or in the N° 6. 7. 8. and 9. either a flat or a ſharp 6th.

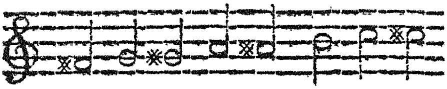


fundamental N° 8. fundamental N° 10.

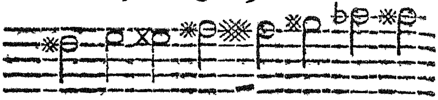
This note of latitude is common to all tubes, trumpets, horns, &c.

The following notes mark the aſcending ſeries of the ſounds of this larger ſyſtem, omitting the fundamentals, and giving only thoſe which are more eaſily obtained.

The numerical figures ſhew from which pipe the notes were produced.



Fifths above the ſuppoſed fundamentals, produced by a gentle blaſt.



Tierces, or tenths, above the ſuppoſed fundamentals, produced by a ſtronger blaſt.

VII. *Deſcription*

VII. *Deſcription of a new Dipping-needle. By Mr.* J. Lorimer, *of* Penſacola, *in a Letter to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR, Penſacola, Sept. 13, 1773.

Redde, Feb. 2, 1775.

WHENEVER any one meets with a *terrella*, or ſpherical loadſtone, the firſt thing he does is to find out its poles; and having once diſcovered them, he knows immediately how any ſmall bit of needle will be affected, if it is placed upon any part of the ſurface of that *terrella*. The poles are moſt readily diſcovered by trying where the filings of iron, or a ſmall bit of needle, will ſtand erect upon the *terrella*; and this is generally found to be upon two points which are diametrically oppoſite to one another. But the magnetic poles of the earth ſeem to be ſituated obliquely to one another (ſee the Berlin Memoirs, 1757); but where they are actually ſituated is hitherto unknown; whether they are upon land or water; or in either caſe, whether we can come nigh to them. Yet be theſe things as they may, it appears evident to me, that accurate obſervations, made as near to theſe magnetic poles as poſſible, with a good dipping-needle, are the ſureſt way to complete the magnetic theory of this globe, analogous to the method we purſue in examining the *terrella*. But as all the dipping-needles which I had ſeen, appeared to me to be very ill calculated, for the ſea ſervice at leaſt, I contrived one upon a different plan in 1764, and had it executed before I left England, by Mr. SISSON. I have called it an Univerſal Magnetic Needle, or Obſervation Compaſs; becauſe I can by it take the dip and amplitude, and even the azimuth, with only one aſſiſtant, to take the altitude for me. The needle is of the ſame ſhape and ſize nearly as thoſe uſed now for the compaſſes of the royal navy, and plays vertically upon its own axis, which has two conical points, ſlightly ſupported in two correſponding hemiſpherical (a) ſockets, which are inſerted into the oppoſite ſides of a ſmall upright braſs paralellogram, about one inch and a half broad and ſix inches high. Into this parallelogram is fixed, at right angles, a ſlender braſs circle, about ſix inches diameter, ſilvered and graduated to every half degree, upon which the needle ſhows the dip, by a *vernier* if you chooſe; and this, for the ſake of diſtinction, I ſhall call the circle of magnetic inclination. This braſs parallelogram, and conſequently the circle of inclination, alſo turns horizontally upon two other pivots, the one above and the other below, with correſponding ſockets in the parallelogram. Theſe pivots are fixed in a vertical braſs circle, of the breadth and thickneſs of two-tenths of an inch, and of ſuch a diameter, as to allow the circle of inclination and the parallelogram to move freely round within it. This ſecond circle I ſhall call the general meridian. It is not graduated, but has a ſmall braſs weight fixed to the lower part of it, to keep it upright; and the circle itſelf is ſcrewed, at right angles, into another circle, of equal internal diameter, of the ſame thickneſs, and twice the breadth, which is ſilvered and graduated on the upper ſide to every half degree. It repreſents the horizon, as it ſwings freely upon gimbols, and is always nearly parallel to it. The whole is contained in a neat mahogany box, of an octagon figure, with a glaſs plate at top and one on each ſide, for about two-thirds down. That part of the frame which contains the glaſs lifts off occaſionally. The whole box turns round upon a ſtrong braſs center, fixed in a double plate of mahogany, glewed together croſs-ways, to prevent its warping or ſplitting; and this again is ſupported by three braſs feet, ſuch as are uſed for the caſes of table knives, froſted that they may not eaſily ſlip, if the veſſel ſhould have any conſiderable motion. It has another ſquare deal box to lock it up in, to preſerve the glaſs, &c. when it is not wanted for uſe.

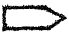
(a) Mr. SISSON thinks, that theſe ſockets were conical as well as the ends of the axis, but more obtuſe than them; which ſeems moſt likely to be the caſe, as they ſeem much more likely to anſwer well than hemiſpherical ſockets.

The uſe of this inſtrument is very plain, as the inclination or dip is at any time apparent from inſpection only, and alſo the variation, if the frame is turned round till the great vertical circle lies exactly in the plane of

the

the true meridian: for the circle of inclination, being always in the needle's vertical plane, the edge of it will evidently point out upon the horizon, the variation E. or W. But at ſea, when there is not too much motion, you turn the frame round, till the vertical circle is in the plane of the Sun's rays; that is, till the ſhadow of the one ſide of it juſt covers the other, and the edge of the circle of inclination will then give the magnetic amplitude, if the Sun is riſing or ſetting; but the azimuth at all other times of the day, and the true amplitude or azimuth being found in the uſual way, the difference is the variation. If the motion is conſiderable, obſerve the extremes of the vibration, and take the mean for your magnetic amplitude or azimuth. When the Sun does not ſhine ſo bright as to give a ſhadow, you can ſet the braſs circle in a line with his body, if he is at all viſible by your eye. The principal advantage at firſt aimed at in this compaſs, was to contrive a dipping-needle, which ſhould be ſufficient for making obſervations at ſea. As thoſe needles, to be of uſe, muſt be placed, by ſome means or other, in ſuch a manner as that all their vibrations ſhall be made in the true magnetic meridian; North and South, otherwiſe they are good for nothing. For if one of them is placed at right angles, acroſs the magnetic line, it will ſtand perpendicularly up and down in any part of the world; the leaſt dip, therefore, is always in this magnetic line. But the only method of ſetting a dipping-needle at ſea, was to place it in a line with the common compaſs needle; and this muſt be very inaccurate, if they are at any conſiderable diſtance one from the other; or if they were near, the two needles would influence one another, and neither of them could be true: nay, ſuppoſing them for once to be properly placed in this line, the leaſt motion of the ſhip throws them out again. But this inſtrument has a conſtant power in itſelf, not only of ſetting itſelf in the proper poſition, but alſo of keeping itſelf ſo; or of reſtoring itſelf to the ſame ſituation, if at any time it has loſt it; and it is curious to ſee how, by its double motion, it counteracts, as it were, the rolling motion of the veſſel. I have only one thing farther to obſerve, that as it is impoſſible for human hands to make any inſtrument mathematically true, ſo when we have two graduations to look to, as in the preſent caſe, one on the North, and the other on the South of the needle, we ought to attend to both, and take the medium for the true dip or variation pretty nearly. But in this compaſs there is another method of examining the obſervations. Take a good artificial magnet, and on the outſide of the compaſs-box, point one end of it towards the needle, and by moving your magnet you may thus guide the north-end of the needle round to the ſouth; or, *vice verſâ*, without opening your compaſs-box. The magnet being then laid aſide, the needle will come to its true poſition, after a few vibrations: but as both the needle and the circle of inclination are now reverſed(b), it will not point exactly to the ſame diviſion as before; yet a mean of the two will be the truth, as nearly, I believe, as it is poſſible for any inſtrument to give it.

(b) Mr. LORIMER means, that the magnet ſhould be applied in ſuch manner as to turn the parallelogram and circle of inclination half way round horizontally, ſo that that end of the axis of the needle which before pointed to the weſt, ſhall now point to the eaſt.

Quere 1ſt. May not a part of this ſmall difference be attributed to the direction of the magnetic influence (whatever that be) in the ſteel bar? and if ſuch an experiment could be tried upon the preſent azimuth compaſſes, is it not probable, that the variation in them would be at leaſt as ſenſible? Quere 2d. May not this be the cauſe that two of the beſt of them will differ a ſmall matter from one another? Quere 3d. Would the ends of the needle being made thus , inſtead of the ſquare form be, in ſome meaſure, a remedy for this ſmall variation?

I am, moſt reſpectfully, SIR,

Your moſt obedient humble ſervant,

J. LORIMER.

VIII. *Bill*

VIII. *Bill of Mortality for* Cheſter *for the Year* 1773. *By* J. Haygarth, *M. D. F. R. S.*

Redde, Feb. 2, 1775.

THAT Cheſter is healthy to a very remarkable degree, is ſtill more clearly evinced from the following tables, than in the regiſter of laſt year. In 1772, one half of the inhabitants appeared to arrive at 20 years of age; a fact which ſeemed very ſurpriſing when compared with the proportional mortality in other towns, both of a larger and leſs ſize. But, according to this year's regiſter, one half have lived to be 36 years old. In 1772, one in 15 and 3-4ths had lived to above 80, and this year 1 in 13. Theſe are very uncommon inſtances of longevity for ſo large a proportion of the inhabitants. The inhabitants of St. Michael's pariſh were numbered to be 618, of whom this year ten have died; that is, a leſs proportion than 1 in 61. If the inhabitants of the whole city were numbered with the ſame accuracy as thoſe of St. Michael's, many important concluſions, both medical and political, might with certainty be deduced from the bill of mortality. The regiſter of burials in the nine pariſhes are kept ſeparate; hence, by comparing the number of inhabitants in each pariſh with the burials in each, for a period of years, we may, on the moſt evident foundation, diſcern

which

which part of the town is moſt healthy. In a political view, ſuch an account would furniſh the beſt means of demonſtrating the accuracy of a table of the probabilities of life, formed from the regiſter, and ſupply unerring *data* for calculating annuities, the value of reverſionary payments, and aſſurances on lives. Such an old town as Cheſter, where the number of inhabitants has for many years ſuffered little variation, and where the births and burials are nearly equal, is peculiarly well fitted to furniſh this important information. At the requeſt of Dr. PRICE, author of the very ingenious eſſays on annuities, &c. an improvement is made in the firſt table, by continuing the diviſion of lives into periods of five years, from 50 till 80, and from that age to the extremity of life, by marking exactly the number dyiug in every year; becauſe for want of ſuch a regiſter, the law, according to which life waſtes after 80, is at preſent almoſt totally unknown, and the values of annuities on ſingle and joint lives, incapable of being calculated with any tolerable exactneſs, beyond 70 or 75. The following tables confirm the obſervation, that women live longer than men. Of thoſe who have lived to above 80, only 10 are males, and 17 females; the number of widowers this year is 17, of widows 44. The table of diſeaſes of different ages (N° II.) confirms in general the obſervations of laſt year. It is evident that no epidemic viſited this place in 1773; not one died of the meaſles, or miliary fever, and the 10 who ſunk under the chinkcough had probably lingered under the diſeaſe ſince the former year, towards the end of which it ceaſed to be epidemic. Only one has died of the natural ſmall-pox; twelve were inoculated in Cheſter, during this year, and all recovered. In order to determine the utility of inoculation, it is neceſſary to aſcertain, by an induction of facts, to what proportion of thoſe who are infected, this diſeaſe is fatal in the natural way. I have received a very authentic account of the following fact, which is perhaps the more curious and inſtructive, as no medical practitioner whatever viſited any of the patients during the whole diſeaſe: at Kelſall and Aſhton, two ſmall Cheſhire villages in this part of the county, on an eminence of a dry abſorbent ſand rock, 69 perſons have had the ſmall-pox during the laſt ſeven months; of whom 12 have died, that is, 1 in 5 and 3-4ths. In confirmation of laſt year's obſervation it is proper to remark, that between the ages of 15 and 50, more have died of conſumptions this year, than of all other diſeaſes.

The IIId table ſhews, at one view, what diſeaſes were moſt fatal in each month.

TABLE I. Deaths, Ages and Conditions.

Ages.	Males.	Females.
Under 1 month —	6	4
Between 1-2 months	11	5
2-3	4	6
3-6	8	8
6-9	1	3
9 months and 1 year	2	8
1-2 years old	5	12
2-3	6	5
3-4	4	6
4-5	1	5
5-10	3	7
10-15	2	2
15-20	7	4
Total of the above ages,	60	75

Ages.	Batchelors.	Huſbands.	Widowers.	Maids.	Wives.	Widows	Total.
20-25	6	1		4	1		12
25-30	5	3		1	6		15
30-35	1	3		4	3		11
35 40	1	5		1	14		21
40-45	1	9	1	2	6	1	20
45-50		10	1	2	1	3	17
50-55	1	3	1	3	5	2	15
55-60		3	1		6	3	13
60-65	2	4	2	2	5	2	17
65-70		4	1	1	2	5	13
70-75	1	5	3		5	10	24
75-80			2	2	1	7	12
80			2	1		2	5
81	1						1
82		1	1			1	3
83						2	2
84					1	1	2
85			1				1
86						1	1
87		1		1		2	4
88					1		1
90						1	1
92		1				1	2
97			1				1
98					1	1	2
106	1						1
Total of ages and conditions,	20	53	17	24	58	45	217

Total of ages under 20 years, { Males, 60 / Females, 75 } 135

Total, 352

TABLE

DISEASES. TABLE II. Diſeaſes of different Ages.

I. FEBRILE DISEASES.	Under 1 year.	Betw. 1 & 2.	2-3.	3-5.	5-10.	10-15.	15-20.	20-30.	30-40.	40-50.	50-60.	60-70.	70 80	80 90.	90-100.	100 & upw.	Total.
Fever (CULLEN'S GENERA) 5 6	1		1	5	3	1	1	2	6	4	4	2	3				33
Impoſthume (G. 7.)							1										1
Angina pectoris (G. 7.)										1							1
Mortification (G. 7.)								2	1	1	1	1	2				8
Quinſey (G. 10.)	1																1
Infl. of the bowels (G. 16.)									3			1					4
Gravel and Stone (G. 19.)													1				1
Teething (Sauv. G. 198.)	1	1															2
Small-Pox (G. 26.)				1													1
Thruſh (G. 33.)	1																1
Conſumption (G. 35.)	2	1	3	1	2	3	9	15	14	16	7						73
Hæmorrhage (G. 37.)								1									1
II. NERVOUS DISEASES.																	
Sudden death (G. 40 ?)								1	1	2		2	1				7
Palſy (G. 41.)									1	1	2	2	2				8
Swoon ? (G. 42.)									1								1
Indigeſtion (G. 43.)										2	3						5
Convulſions (G. 48. 50.)	50	8	3	3				1	2	1		1					69
Aſthma (G. 52.)									1	1	4	4	6				16
Chinkcough (G. 53.)	3	3	2	1	1												10
Colic (G. 55.)					1					1		1					3
Looſeneſs (G. 57.)	1																1
Inſanity (G. 63.)								1		1		1					3
III. DISEASES of the HABIT.																	
Weakneſs of infancy (G. 65.)	4	4	2	3													13
Decay of age (G. 66.)											5	12	18	20	6	1	62
Dropſy G. 71. 75.								1	1	4	2	2	3				13
Dropſy of the brain (G. 72.)					1												1
Jaundice (G. 87.)									1	1							2
IV. LOCAL DISEASES.																	
Cancer (G. 114.)					1								1				2
Unknown diſeaſes.	1							1									2
Caſualties.				2	1			2		1		1					7
Total,	65	17	11	16	10	4	11	27	32	37	28	30	37	20	6	1	352

MORBORUM GENERA.
CLASSIS I. PYREXIÆ.
G. 5. 6. Typhus. Synochus.
G. 7. Apoſtema.
G. 7. Apoſtema mediaſtini.
G. 7. Gangræna.
G. 10. Cynanche.
G. 16. Enteritis.
G. 19. Nephritis.
Sauv. G. 198. Odontalgia dentitionis.
G. 26. Variola.
G. 33. Aphtha.
G. 35. Phthiſis.
G. 37. Menorrhagia *lochialis.*
CLASSIS II. NEUROSES.
G. 40. Apoplexia?
G. 41. Paralyſis.
G. 42. Synchope? *(puerperalis.)*
G. 43. Dyſpepſia.
G. 48, 50. Convulſio. Epilepſia.
G. 52. Aſthma.
G. 53. Pertuſſis.
G. 55. Colica.
G. 57. Diarrhæa.
G. 63. Mania.
CLASSIS III. CACHEXIÆ.
G. 65. Tabes, atrophia infantilis.
G. 66. Atrophia ſenilis.
G. 71. 75. Aſcites. Anaſarca.
G. 72. Hydrocephalus.
G. 87. Icterus.
CLASSIS IV. LOCALES.
G. 114. Cancer.

TABLE III. Diſeaſes of different Months.

DISEASES I. Febrile Diſeaſes.	Jan.	Feb.	March	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Total.
Fever (CULLEN'S GENERA 5, 6.)	1	4	6	4		5	1	2		3	1	6	33
Impoſthume (G. 7.)												1	1
Angina pectoris (G. 7.)						1							1
Mortification (G. 7.)		1		1		1	1			1	1	2	8
Quinſey (G. 10.)				1									1
Infl. of the bowels (G. 16.)				1	1	1						1	4
Gravel and ſtone (G. 19.)					1								1
Teething (Sauv. 198)	1					1							2
Small-pox (G. 26.)										1			1
Thruſh (G. 33.)					1								1
Conſumption (G. 35.)	5	11	8	6	10	5	5	3	1	1	8	10	73
Hæmorrhage (G. 37.)		1											1
II. NERVOUS DISEASES.													
Sudden death (G. 40.)						1		1		2		3	7
Palſy (G. 41.)	1	1	2	2							2		8
Swoon (G. 42.)			1										1
Indigeſtion (G. 43.)			1	2				1	1				5
Convulſions (G. 48, 50.)	8	7	3	11	6	7	4	6	3	2	3	9	69
Aſthma (G. 52.)	2	3	5	2					2			2	16
Chinkcough (G. 53.)	4	3		1				1		1			10
Colic (G. 55.)		1									2		3
Looſeneſs (G. 57.)											1		1
Inſanity (G. 63.)			1	1	1								3
III. DISEASES of the HABIT.													
Weakneſs of infancy (G. 65.)		2	1	1	2	2	1			1		3	13
Decay of age (G. 66.)	3	10	7	8	8	5	2		3	4	6	6	62
Dropſy (G. 71, 75.)	2		1	1	2	1		2	1		2	1	13
Dropſy of the brain (G. 72.)			1										1
Jaundice (G. 87.)									1	1			2
IV. LOCAL DISEASES.													
Cancer (G. 114.)	1									1			2
Unknown diſeaſes.					1						1		2
Caſualties.		1				3						3	7
Total,	28	45	37	42	33	33	14	16	12	18	27	47	352

GENERAL BILL of MARRIAGES, BAPTISMS, and BURIALS.
For the YEAR 1773.

Marriages, 133. Baptiſms, {Males, 216 / Females, 186} 402. Deaths, {Males, 150 / Females, 202} 352.

IX. *Experiments on a new Colouring Subſtance from the Iſland of* Amſterdam *in the* South Sea. *Made by Mr.* Peter Woulfe, *F. R. S. at the Deſire of Sir* John Pringle, *Bart. P. R. S.*

Redde, Feb. 2, 1775.

THIS ſubſtance is of a light bright orange colour; has a peculiar, though not a ſtrong, ſmell; and, when handled, gives a yellow ſtain to the ſkin, which does not readily waſh out with ſoap and water. Put on a red hot iron, it ſmoaks, melts, and catches fire, leaving a *caput mortuum*. When boiled with water, it gives the liquor only a ſlight yellow tinge, which is but little heightened by the addition of a fixed alkaly; therefore the colouring part of this ſubſtance is inſoluble in water. Oil of vitriol put to it becomes of a red orange colour; but, when the acid is drained off, the *reſiduum* appears purple. Annotto, treated in the ſame manner, gives a blue colour. Spirit of wine, æther, fixed and volatile alcalies, as alſo ſoap, diſſolve the colouring part of this ſubſtance. To determine the quantity of colouring matter which it contains, two drams were digeſted in a mattraſs, with four ounces of rectified ſpirit of wine; the ſolution being filtered aſſumed a rich deep yellow colour, like a ſtrong ſolution of ſaffron or gumbouge with the ſame ſpirit; what remained in the filter was digeſted a ſecond time, with four ounces of freſh ſpirit of wine, and the liquor filtered; this ſolution was much weaker than the firſt. The undiſſolved part remaining in the filter after this ſecond ſolution was digeſted, a third time, with four ounces of freſh ſpirit; but the ſolution was now quite weak, and of a very pale yellow colour. The *reſiduum* being now deprived of its colouring portion, was ſlowly dried, when it appeared of a very pale yellow colour, felt as ſoft as ſtarch between the fingers, and weighed forty two grains; ſo that two thirds nearly of this colouring ſubſtance are ſoluble in ſpirit of wine; the undiſſolved part is not ſoluble in water, acids or alkalies. Put on a red hot iron, it ſmoaks and catches fire without melting, leaving a *caput mortuum*, and gives a ſmell ſimilar to that ariſing from common vegetable matter. The firſt ſolution in ſpirit of wine, after ſtanding twenty-four hours, depoſits ſome of its colour in the form of minute ſpiculine cryſtals, of an orange colour. The ſecond and third ſolutions let fall none of their colour. The firſt ſolution, dropped on paper, tinges it of a bright orange colour, the ſecond gives a lively yellow colour, and the third a pale yellow. The firſt ſolution, ſufficiently diluted with ſpirit of wine, makes a bright yellow ſtain on paper, no way inclining to an orange, but exactly reſembling that made by the ſecond ſolution; hence it ſeems probable, that an orange colour is only a deep yellow. Vitriolic æther readily diſſolves the colouring part of this ſubſtance, and affords ſolutions of nearly the ſame colour as thoſe made with ſpirit of wine. Oil of turpentine diſſolves but a ſmall portion of it, and acquires only a pale yellow colour. A ſolution of fixed alkaly in water, digeſted with this ſubſtance, diſſolves a large portion of its colouring part, and the ſolution is of a browniſh yellow colour. Volatile ſpirit of *ſal ammoniac*. ſeems to diſſolve a larger portion of it than the fixed alkaly, and the ſolution is of a reddiſh orange colour. A ſolution of ſoap in water, boiled with this ſubſtance, likewiſe diſſolves its colouring part. All the foregoing ſolutions, except that in oil of turpentine, which was not tried, die ſilk, cloth, and linen, of various ſhades of yellow and orange; but theſe colours are diſcharged, by boiling the dyed ſubſtances for ſome time in ſoap and water. This colour can, therefore, be of uſe only in dying ſilk and wool, for which purpoſe we are already furniſhed with good dyes. Few colours go ſo far in dying as this new ſubſtance, and none dye ſo ſpeedily, eſpecially when ſoap and water are uſed as the ſolvent; for a dip or two will dye cloth or ſilk of a lively yellow colour, when put into the mixture whilſt hot. Soap and water may be perhaps uſed with advantage, as the ſolvent for ſeveral other colours.

From the foregoing experiments it appears, that this colouring ſubſtance, upon which they have been made, is of the reſinous kind, and has a good deal of affinity with *annotta*.

X. *Experiments and Obſervations on the* Gymnotus Electricus, *or Electrical Eel. By* Hugh Williamſon, *M.D. Communicated by* John Walſh, *Eſq. F. R. S.*

TO JOHN WALSH, ESQ.

SIR, London Feb. 7, 1775.

Redde, Feb. 9, 1775.

AS the electrical eel has lately engaged the public attention, and yours in particular, I have taken the liberty of ſending you ſome experiments which I made on that fiſh: they are the ſame that I had the pleaſure of ſhewing you laſt winter, on my arrival from Penſylvania. If you apprehend they may tend to caſt any light on that curious part of natural hiſtory, or to gratify the curioſity of the public, be pleaſed to make any uſe of them you may think proper. Beſides my own ſuperficial acquaintance with the ſubject of electricity, of which I am very conſcious, there are other circumſtances that may help to apologize for the imperfect ſtate in which theſe experiments appear. The eel being ſickened by the change of climate, its owner refuſed to let us take it out of the water, for the purpoſe of making experiments, on reaſonable terms; and there were many experiments which I could not make on it in the water, to my own ſatisfaction. While I made theſe experiments, the eel was kept in a large veſſel, ſupported by pieces of dry timber, about three feet above the floor. Perhaps it may deſerve notice, that a ſmall hole being bored in the veſſel in which the eel was ſwimming, one perſon provoked the eel ſo as to receive a ſhock; another perſon at the ſame time, not in contact with him, but holding his finger in the ſtream that ſpouted from the veſſel, received a ſhock alſo in that finger. From this and ſundry other experiments, I am induced to believe, that the *gymnotus* has powers greatly ſuperior to, or rather different from, thoſe of the *torpedo*, which you have examined with ſo much attention. I have the honour to be, SIR,

Your moſt obedient

and very humble ſervant,

HUGH WILLIAMSON.

Philadelphia, Sept. 3, 1773.

SOME weeks ago, a ſea-faring man brought to this city a large eel, that had been caught in the province of Guiana, a little to the weſtward of Surinam. It had the extraordinary power of communicating a painful ſenſation, like that of an electrical ſhock, to people who touched it, and of killing its prey at a diſtance. As I have not heard that any other eel of this kind has ever been carried to any of our continental colonies, or that any of them have been ſeen in Europe, I ſhall take the liberty, after I have given a ſhort deſcription of the fiſh, to relate ſuch experiments as I made, or aſſiſted in making, in hopes of diſcovering by what means it produced the effects I have mentioned. The eel was three feet ſeven inches long, and about two inches thick near the head. On a tranſient view, it reſembled one of our common eels both in ſhape and colour; but its head was flat and its mouth wide, like that of a cat-fiſh, without teeth. A fin, which was above two inches broad, extended along its belly, from the point of its tail to within ſix inches of its head. This fin was almoſt an inch thick where it adhered to the body; the upper part of it was muſcular, but of a very different texture from the muſcular part of the body; the difference was obvious to the touch, for I had no opportunity of making any obſervations by diſſecting the ſubject. It was a native of freſh water, and breathed at the interval of three or four minutes, by lifting its head to the ſurface.

EXPERIMENTS.

1. On touching the eel with one of my hands, I perceived ſuch a ſenſation in the joints of my fingers as I received on touching a prime conductor or charged phial, when no circle was formed; or ſuch as I have received, when a few ſparks of the electric fluid have been conveyed through my fingers only. 2. On touching the eel more roughly, I perceived a ſimilar effect in my wriſt and elbow. 3. Touching the eel

with an iron rod, twelve inches long, I perceived the like ſenſation in the joints of the thumb and fingers with which I held the metal. 4. While another perſon provoked the eel by touching it, I put my hand into the water at the diſtance of three feet, and felt ſuch a ſenſation in the joints of my fingers as when I had touched the eel, but not ſo painful. 5. Some ſmall fiſhes were thrown into the water where he was ſwimming; he killed them immediately, and ſwallowed them. 6. A cat-fiſh (a), that was at leaſt one inch and an half thick, was thrown into the water where the eel was ſwimming; he killed it alſo, and attempted to ſwallow it, but could not. 7. In order to diſcover whether the eel killed thoſe fiſh by an emiſſion of the ſame fluid with which he affected my hand when I had touched him, I put my hand into the water, at ſome diſtance from the eel; another cat-fiſh was thrown into the water; the eel ſwam up to it, but preſently turned away, without offering any violence. After ſome time he returned; when, ſeeming to view it for a few ſeconds, he gave it a ſhock, by which it inſtantly turned up its belly, and continued motionleſs; at that very inſtant I felt ſuch a ſenſation in the joints of my fingers as in experiment 4. 8. A third cat-fiſh was thrown into the water, to which the eel gave ſuch a ſhock, that it turned on its ſide, but continued to give ſigns of life. The eel ſeeming to obſerve this, as it was turning away, immediately returned, and ſtruck it quite motionleſs. I could eaſily perceive that the laſt ſhock was more ſevere than the former. The eel never attempted to ſwallow any of thoſe fiſh after the firſt, though he killed many of them; and I always obſerved, that when he was going to kill one, he ſwam directly up to it, as if he was going to bite it; that when he came up, he ſometimes pauſed before he gave the ſhock, at other times he gave the ſhock immediately. When we removed any of thoſe cat-fiſh, though apparently dead, into water in another veſſel, they preſently recovered. Fiſh that are ſtunned by a ſmall electrical ſhock were found to recover in the ſame manner. 9. Touching the eel, ſo as to provoke it, with one hand, and at the ſame time holding my other hand in the water, at a ſmall diſtance, a ſhock paſſed through both my arms, as in the caſe of the Leyden experiment. 10. I put the end of a wet ſtick into the water, and holding it with one hand, I touched the eel with the other; a ſhock paſſed through both arms as before. 11. Taking another gentleman in company by the hand, he touched the eel, while I held one of my hands in the water; the ſhock paſſed through us both. 12. Inſtead of putting my hand into the water, at a diſtance from the eel, as in the laſt experiment, I touched its tail, ſo as not to offend it, while my aſſiſtant touched its head more roughly; we both received a ſevere ſhock. 13. Eight or ten perſons, taking hands, ſtood in a circular form; the firſt in the ſeries touched the eel, while the laſt put his hand into the water, at ſome diſtance from it; they all received a gentle ſhock. 14. The above experiment was repeated with no other variation than that the laſt perſon touched the eel's tail, while the firſt touched its head; they all received a ſevere ſhock. 15. Another gentleman and myſelf, holding the extremities of a braſs chain, one of us put his hand into the water, while the other touched the eel, ſo as to offend it; the ſhock paſſed through us both. 16. I wrapped a ſilk handkerchief round my hand, and touched the eel with it, but received no ſhock; although another gentleman felt the ſhock, who, at the ſame time, put his hand into the water, at ſome diſtance from the eel. 17. A great variety of other experiments were made by two perſons, one touching the eel near its head, the other putting his hand into the water, or touching it near the tail, forming a communication at the ſame time between their hands, which were out of the water, by pieces of charcoal, rods of iron or braſs, a piece of dry wood, glaſs, ſilk, &c. The uniform reſult of all thoſe experiments was, that whatever uſes to convey the electrical fluid would alſo convey the fluid diſcharged by the eel; and *vice verſâ*, a braſs chain, that had very many links in it, would not convey it, unleſs when the ſhock was ſevere, or the chain tenſe. 18. One of the company being inſulated on glaſs bottles, received ſeveral ſhocks from the eel; but he exhibited no marks of a *plus* ſtate of electricity, nor would cork-balls, ſuſpended by ſilken threads, give any marks of it, either when they were ſuſpended over the eel's back, or touched by the inſulated perſon at the inſtant he received the ſhock. 19.

(a) The Bayre de rio of Marcgrave.

A perſon, holding a phial in one hand properly lined and coated for electrical experiments, put his hand to the tail of the fiſh, while an aſſiſtant, holding a ſhort wire in one hand that communicated with the inſide of the phial, graſped the fiſh near its head, ſo as to receive a ſevere ſhock in his hand and arm, but it paſſed no further.

20. Two pieces of braſs wire, about the thickneſs of a crow's quill, were ſcrewed, in oppoſite directions, into a frame of wood, ſo as to come within leſs than the hundredth part of an inch of contact; they were rounded at the point. I held the remote end of one of thoſe wires, while an aſſiſtant held the other; in the mean while, one of us putting his hand into the water near the eel, the other touched it ſo as to receive a ſhock. We repeated this experiment fifteen or twenty times with different ſucceſs: when the points of the wires were even ſcrewed aſunder, to the fiftieth part of an inch, the ſhock never paſſed in the circle; but when they were ſcrewed up within the thickneſs of double-poſt paper, the ſhocks, ſuch of them as were ſevere, would paſs through us both; in which caſe, they doubtleſs leaped from the point of one wire to the other, though we were not ſo fortunate as to render the ſpark generally viſible. But it ſhould be obſerved, that the eel on which we made theſe experiments, was not eaſily provoked, and appeared to be in bad health. I have frequently paſſed my hand along its back and ſides from head to tail, and have lifted part of its body above the water, without tempting it to make any defence. Dr. BANCROFT tells us, that ſuch eels in

Guiana have ſhocked his hand at the diſtance of ſome inches from the ſurface of the water. Perhaps fire emitted by eels lately taken, might be rendered viſible.

From the above experiments it appears: 1. That the Guiana eel has the power of communicating a painful ſenſation to animals that touch or come near it. 2. That this effect depends entirely on the will of the eel; that it has the power of giving a ſmall ſhock, a ſevere one, or none at all, juſt as circumſtances may require. 3. That the ſhock given, or the painful ſenſation communicated, depends not on the muſcular action of the eel, ſince it ſhocks bodies in certain ſituations at a great diſtance; and ſince particular ſubſtances only will convey the ſhock, while others, equally elaſtic or hard, refuſe to convey it. 4. That the ſhock muſt therefore depend upon ſome fluid, which the eel diſcharges from its body. 5. That as the fluid diſcharged by the eel affects the ſame parts of the human body that are affected by the electric fluid; as it excites ſenſations perfectly ſimilar; as it kills or ſtuns animals in the ſame manner; as it is conveyed by the ſame bodies that convey the electric fluid, and refuſes to be conveyed by other bodies that refuſe to convey the electric fluid, it muſt alſo be the true electrical fluid; and the ſhock given by this eel muſt be the true electrical ſhock.

XI. *An Account of the* Gymnotus Electricus, *or Electrical Eel. In a Letter from* Alexander Garden, *M. D. F. R. S. to* John Ellis, *Eſq. F. R. S.*

Charles-Town, South Carolina,
Aug. 14, 1774.

SIR,

Redde, Feb. 23, 1775.

A FEW days ſince, I went to ſee ſome very curious fiſh, which were brought here about nine or ten weeks ago from Surinam; and I was both ſurprized and delighted to obſerve their ſtrange ſhape, and experience their wonderful properties. I had before received ſome vague account of ſuch a fiſh; but I always thought, that much of what I heard was fabulous. There are five of theſe fiſhes now here, of different ſizes, from two feet in length to three feet eight inches. The following deſcription was made out from the longeſt and largeſt. It might have been much more accurate, if there had been a poſſibility of handling the fiſh, and examining it leiſurely; or if I could have had a dead ſpecimen, as many things relating to the internal and external ſtructure could in that caſe have been more exactly aſcertained. But this fiſh hath the amazing power of giving ſo ſudden and ſo violent a ſhock to any perſon that touches it, that there is, I think, an abſolute impoſſibility of ever examining accurately a living ſpecimen,

ſpecimen; and the perſon who owns them rates them at too high a price (not leſs than fifty guineas for the ſmalleſt) for me to get a dead ſpecimen, unleſs one ſhould die by accident; if that ſhould happen, you may depend on having a more exact and accurate account for the Society.

GEORGE BAKER, mariner, who brought them here, intends to carry them to England; but as it is very uncertain whether they will arrive in health and all alive, I have recommended to him to get a ſmall caſk of rum, with a large bung, into which he may put any of them that may die, and ſo preſerve them for the inſpection and examination of the curious when he arrives.

The largeſt of theſe fiſh was three feet eight inches in length, when extending itſelf moſt, and might have been from ten to fourteen inches in circumference about the thickeſt part of his body. The head is large, broad, flat, ſmooth, and impreſſed here and there with holes, as if perforated with a blunt needle, eſpecially towards the ſides, where they are more regularly ranged in a line on each ſide. The *roſtrum* is obtuſe and rounded. The upper and lower jaws are of an equal length, and the gape is large. The noſtrils are two on each ſide; the firſt large, tubular, and elevated above the ſurface; and the others ſmall, and level with the ſkin, placed immediately behind the verge of the *roſtrum*, at the diſtance of an inch aſunder. The eyes are ſmall, flattiſh, and of a blueiſh colour, placed about three quarters of an inch behind the noſtrils, and more towards the ſides of the head. The whole head ſeems to be well ſupported;

but whether with bones or cartilages, I could not learn. The body is large, thick, and roundiſh, for a conſiderable diſtance from the head, and then gradually grows ſmaller, but at the ſame time deeper, or becomes of an *acinaciform* ſhape, to the point of the tail, which is rather blunt. There are many light-coloured ſpots on the back and ſides of the body, placed at conſiderable diſtances in irregular lines, but more numerous and diſtinct towards the tail. When the fiſh was ſwimming, it meaſured ſix inches in depth near the middle, from the upper part of the back to the lower edge of the fin, and it could not be more than two inches broad on the back at that place. The whole body, from about four inches below the head, ſeems to be clearly diſtinguiſhed into four different longitudinal parts or diviſions. The upper part or back is roundiſh, of a dark colour, and ſeparated from the other parts on each ſide by the *lateral lines*; which, taking their riſe at the baſe of the head, juſt above the pectoral fins, run down the ſides, gradually converging, as the fiſh grows ſmaller, to the tail, and make ſo viſible a depreſſion or furrow in their courſe, as to diſtinguiſh this from the ſecond part or diviſion, which may be properly called the body, or at leaſt, appears to be the ſtrong muſcular part of the fiſh. This ſecond diviſion is of a lighter and more clear blueiſh colour than the upper or back part, and ſeems to ſwell out ſomewhat on each ſide, from the depreſſion of the lateral lines; but, towards the lower or under part, is again contracted, or ſharpened into the third part, or *carina*. This *carina*, or heel, is very diſtinguiſhable from the other two diviſions, by its thinneſs, its apparent laxneſs, and by the reticulated ſkin of a more grey and light colour, with which it is covered. When the animal ſwims gently in pretty deep water, the rhomboidal reticulations of the ſkin of this *carina* are very diſcernible; but when the water is ſhallow, or the depth of the *carina* is contracted, theſe reticulations appear like many irregular longitudinal *plicæ*. The *carina* begins about ſix or ſeven inches below the baſe of the head, and gradually widening or deepening as it goes along, reaches down to the tail, where it is thinneſt. It ſeems to be of a ſtrong muſcular nature. Where it firſt takes its riſe from the body of the fiſh, it ſeems to be about one inch or one inch and an half thick, and is gradually ſharpened to a thin edge, where the fourth and laſt part is ſituated; *videlicet*, a long, deep, ſoft, wavy fin, which takes its riſe about three or four inches at moſt below the head, and runs down along the ſharp edge of the *carina* to the extremity of the tail. Where it firſt riſes it is not deep, but gradually deepens or widens as it approaches to the tail. It is of a very pliable ſoft conſiſtence, and ſeems rather longer than the body. The ſituation of the *anus* in this fiſh is very ſingular, being placed underneath, and being about an inch more forward than the pectoral fins, and conſequently conſiderably nearer the *roſtrum*. It is a pretty long *rima* in appearance; but the aperture muſt be very ſmall, as the formed excrements are only about the ſize of a quill of a common dunghill fowl. There are two pectoral (if I may call them ſo) fins, placed one on each ſide, juſt behind the head, over the *foramina ſpiratoria*, which are ſmall, and generally covered with a lax ſkin, ſituated in the *axillæ* of theſe fins. Theſe fins are ſmall for the ſize of the fiſh, being ſcarcely an inch in length, of a very thin, delicate conſiſtence, and orbicular ſhape. They ſeem to be chiefly uſeful in ſupporting and raiſing the head of the fiſh when he wants to breathe, which he does every four or five minutes, by raiſing his mouth out of the water. This ſhews that he has lungs and is amphibious, and the *foramina ſpiratoria* ſeem to indicate his having *branchiæ* likewiſe; but this I only offer as a conjecture, not being certain of the fact. I muſt now mention the appearances of a number of ſmall croſs bands, annular diviſions, or rather *rugæ* of the ſkin of the body. They reach acroſs the body down to the baſe of the *carina* on each ſide; but thoſe that croſs the back ſeem to terminate at the lateral lines, where new rings take their riſe, not exactly in the ſame line, and run down to the *carina*. This gives the fiſh ſomewhat of a worm-like appearance; and indeed it ſeems to have ſome of the properties of this tribe, for it has a power of lengthening or ſhortening its body to a certain degree, for its own conveniency, or agreeable to its own inclination. I have ſeen this ſpecimen, which I have meaſured three feet eight inches, ſhorten himſelf to three feet two inches; but beſides this power of lengthening or ſhortening his body, he can ſwim forwards or backwards with apparently equal eaſe to himſelf, which is another property of the vermicular tribe. When he ſwims forward, the undulation or wavy motion of the fin and *carina* begin from the upper part, and move downwards; but when he ſwims backwards, and the tail goes foremoſt, the undulations of the fin begin at the extremity of the tail or fin, and proceed in ſucceſſion from that backwards to the upper part of the body; in either caſe he ſwims equally ſwift. Every now and then the fiſh lays himſelf on one ſide, as it were, to reſt himſelf, and then the four ſeveral diviſions of his body abovementioned are very diſtinctly ſeen; *videlicet*, the vermiform appearance of the two upper diviſions; the retiform appearance of the *carina*; and the laſt, or dark-coloured fin, whoſe rays ſeem to be exceedingly ſoft and flexible, and entirely at the command of the ſtrong muſcular *carina*. When he is taken out of the water, and laid on his belly, the *carina* and fin lye to one ſide, in the ſame manner as the ventral fin of the *Tetraodon* does, when he creeps on the ground. I have been the longer and more particular in the deſcription of the external ſtructure of this animal's body, becauſe, I think, as it is of a moſt ſingular nature, and endowed with ſome amazing properties, even the moſt minute circumſtance I was able to obſerve relating to it ſhould be mentioned.

The perſon to whom theſe animals belong, calls them *Electrical Fiſh*; and indeed the power they have of giving an electrical ſhock to any perſon, or to any number of perſons who join hands together, the extreme perſon on each ſide touching the fiſh, is their moſt ſingular and aſtoniſhing property. All the five we have here are poſſeſſed of this power in a very great degree, and communicate the ſhock to one perſon, or to any number of perſons, either by the immediate touch of the fiſh with the hand, or by the mediation of any metalline rod. The keeper ſays, that when they were firſt caught, they could give a much ſtronger ſhock by a metalline conductor than they can do at preſent. The perſon who is to receive the ſhock muſt take the fiſh with both hands, at ſome conſiderable diſtance aſunder, ſo as to form the communication, otherwiſe he will not receive it; at leaſt I never ſaw any one ſhocked from taking hold of it with one hand only: though ſome have aſſured me, that they were ſhocked by laying one hand on him. I myſelf have taken hold of the largeſt with one hand often, without ever receiving a ſhock; but I never touched it with both hands, at a little diſtance aſunder, without feeling a ſmart ſhock. I have often remarked, that when it is taken hold of with one hand, and the other hand is put into the water over its body, without touching it, the perſon received a ſmart ſhock; and I have obſerved the ſame effect follow, when a number joined hands, and the perſon at one extremity of the circle took hold of, or touched the fiſh, and the perſon at the other extremity put his hand into the water, over the body of the fiſh. The ſhock was communicated through the whole circle, as ſmartly as if both the extreme perſons had touched the fiſh. In this it ſeems to differ widely from the *torpedo*, or elſe we are much miſinformed of the manner in which the benumbing effect of that fiſh is communicated. The ſhock which our Surinam fiſh gives, ſeems to be wholly electrical; and all the phænomena or properties of it exactly reſemble thoſe of the electric *aura* of our atmoſphere when collected, as far as they are diſcoverable from the ſeveral trials made on this fiſh. This ſtroke is communicated by the ſame conductors, and intercepted by the interpoſition of the ſame original electrics, or electrics *per ſe* as they uſed to be called. The keeper of this fiſh informs me, that he catched them in Surinam river, a great way up, beyond where the ſalt water reaches; and that they are a freſh water fiſh only. He ſays, that they are eaten, and by ſome people eſteemed a great delicacy. They live on fiſh, worms, or any animal food, if it is cut ſmall, ſo that they can ſwallow it. When ſmall live fiſhes are thrown into the water, they firſt give them a ſhock, which kills or ſo ſtupifies them, that they can ſwallow them eaſily, and without any trouble. If one of theſe ſmall fiſhes, after it is ſhocked, and to all appearance dead, be taken out of the veſſel where the electrical fiſh is, and put into freſh water, it will ſoon revive again. If a larger fiſh than they can ſwallow be thrown into the water, at a time that they are hungry, they give him ſome ſmart ſhocks, till he is apparently dead, and then they try to ſwallow or ſuck him in; but, after ſeveral attempts, finding he is too large, they quit him. Upon the moſt careful inſpection of ſuch fiſh, I could never ſee any mark of teeth, or the leaſt wound or ſcratch on them. When the electrical fiſh are hungry, they are pretty keen after their food; but they are ſoon ſatisfied, not being able to contain much at one time. An electrical fiſh of three feet and upwards in length cannot ſwallow a ſmall fiſh above three or at moſt three inches and a half long. Since I wrote the above deſcription and remarks, I have had Mr. BANCROFT's Eſſay on the Natural Hiſtory of Guiana put into my hands, in which I find an account of this animal; but, as I think that he has not been very particular in the deſcription of it, I reſolved ſtill to ſend you the above account, that you might judge for yourſelf. I obſerve, that his account or deſcription and mine differ in ſeveral things; and amongſt others, where he ſays, that thoſe fiſh were uſually about three feet in length; but the one, of which I have ſent a ſlight deſcription, was three feet eight inches. This ſmall variation might indeed have happened without any error; but I am told, that ſome of them have been ſeen in Surinam river upwards of twenty feet long, whoſe ſtroke or ſhock proved inſtant death to any perſon that unluckily received it.

I ſhall be on the watch to procure a more accurate knowledge of, and acquaintance with, this animal; and if I can learn any thing farther about it, you may depend on my communicating it.

XII. *Experiments and Obſervations in an heated Room.*

By Charles Blagden, *M. D. F. R. S.*

Redde, Feb. 16, 1774.

ABOUT the middle of January, ſeveral gentlemen and myſelf received an invitation from Dr. GEORGE FORDYCE, to obſerve the effects of air heated to a much higher degree than it was formerly thought any living creature could bear. We all rejoiced at the opportunity of being convinced, by our own experience, of the wonderful power with which the animal body is endued, of reſiſting an heat vaſtly greater than its own temperature; and our curioſity was not a little excited to obſerve the circumſtances attending this remarkable power. We knew, indeed, that of late ſeveral convincing arguments had been adduced, and obſervations made, to ſhew the error of the common opinions on this ſubject; and that Dr. FORDYCE had himſelf proved the miſtake of Dr. BOERHAAVE (a) and moſt other authors, by ſupporting many times very high degrees of heat, in the courſe of a long train of important experiments; with which, and his moſt philoſophical concluſions from them, every lover of ſcience muſt earneſtly wiſh that he may ſoon favour the public. In the mean time time, I am happy in an opportunity of laying before this Society the following ſhort account of ſome of theſe experiments, and of the views with which they were undertaken; for the particulars of which I am obliged to Dr. FORDYCE himſelf.

(a) Elem. Chemiæ, tom. I. p. 277, 278.

DR. CULLEN long ago ſuggeſted many arguments to ſhew, that life itſelf had a power of generating heat, independent of any common chemical or mechanical means; for, before his time, the received opinions were, that the heat of animals aroſe either from friction or fermentation (b). Governor ELLIS in the year 1758 obſerved (c), that a man can live in air of a greater heat than that of his body; and that the body, in this ſituation, continues its own cold. The Abbé CHAPPE D'AUTEROCHE informs us, that the Ruſſians uſe their baths heated to 60° (d) of REAUMUR's thermometer, about 160 of FAHRENHEIT's, without taking notice, however, of the heat of their bodies when bathing. With a view to add further evidence to theſe extraordinary facts, and to aſcertain the real effects of ſuch great degrees of heat on the human body, Dr. FORDYCE tried the following experiments.

(b) To do further juſtice to the philoſophy of this moſt ingenious and reſpectable profeſſor, I muſt here declare, that during my ſtay in Edinburgh, from the year 1765 to 1769, the idea of a power in animals of *generating cold* (that was the expreſſion) when the heat of the atmoſphere exceeded the proper temperature of their bodies, was pretty generally received among the ſtudents of phyſic, from Dr. CULLEN's arguments; in conſequence of which I applied a thermometer, in a hot ſummer day, to the belly of a frog, and found the quickſilver ſink ſeveral degrees: a rude experiment indeed, but ſerving to confirm the general fact, that the living body poſſeſſes a power of reſiſting the communication of heat.

(c) Philoſophical Tranſactions, vol. L. p. 755.

(d) Voy. en Siberie, tom. I. p. 51.

He procured a *ſuite* of rooms, of which the hotteſt was heated by flues in the floor, and by pouring upon it boiling water; and the ſecond was heated by the ſame flues, which paſſed through its floor to the third. The firſt room was nearly circular, about ten or twelve feet in diameter and height, and covered with a dome, in the top of which was a ſmall window. The ſecond and third rooms were ſquare, and both furniſhed with a ſky-light. There was no chimney in theſe rooms, nor any vent for the air, excepting through crevices at the door. In the firſt room were placed three thermometers; one in the hotteſt part of it, another in the cooleſt part, and a third on the table, to be uſed occaſionally in the courſe of the experiment: the frame of this laſt was made to turn back by a joint, ſo as to leave the ball and about two inches of the ſtem quite bare, that it might be more conveniently applied for aſcertaining the heat of the body, and ſeveral other purpoſes.

EXPERIMENT I.

In the firſt room the higheſt thermometer ſtood at 120°, the loweſt at 110°; in the ſecond room the heat was from 90° to 85°; the third room felt moderately warm, while the external air was below the freezing point. About three hours after breakfaſt, Dr. FORDYCE having taken off all his cloaths, except his ſhirt, in the third room, and being furniſhed with wooden ſhoes, or rather ſandals tied on with liſt, entered into the ſecond room, and ſtaid five

minutes

minutes in a heat of 90°, when he began to ſweat gently. He then entered the firſt room, and ſtood in the part heated to 110°; in about half a minute his ſhirt became ſo wet that he was obliged to throw it aſide, and then the water poured down in ſtreams over his whole body. Having remained ten minutes in this heat of 110°, he removed to the part of the room heated to 120°; and after ſtaying there twenty minutes, he found that the thermometer placed under his tongue, and held in his hand, ſtood juſt at 100°, and that his urine was of the ſame temperature. His pulſe had gradually riſen till it made 145 pulſations in a minute. The external circulation was greatly increaſed; the veins had become very large, and an univerſal redneſs had diffuſed itſelf over the body, attended with a ſtrong feeling of heat. His reſpiration, however, was but little affected. Here Dr. FORDYCE remarks, that the moiſture of his ſkin moſt probably proceeded chiefly from the condenſation of the vapour in the room upon his body. He concluded this experiment in the ſecond room, by plunging into water heated to 100°; and, after having been wiped dry, was carried home in a chair; but the circulation did not ſubſide for two hours, after which he walked out in the open air, and ſcarcely felt the cold.

EXPERIMENT II.

In the firſt room the higheſt thermometer varied from 132° to 130°; the loweſt ſtood at 119°. Dr. FORDYCE having undreſſed in an adjoining cold chamber, went into the heat of 119°; in half a minute the water poured down in ſtreams over his whole body, ſo as to keep that part of the floor where he ſtood conſtantly wet. Having remained here fifteen minutes, he went into the heat of 130°; at this time the heat of his body was 100°, and his pulſe beat 126 times in a minute. While Dr. FORDYCE ſtood in this ſituation, a Florence flaſk was brought in, by his order, filled with water heated to 100°, and a dry cloth, with which he wiped the ſurface of the flaſk quite dry; but it immediately became wet again, and ſtreams of water poured down its ſides; which continued till the heat of the water within had riſen to 122°, when Dr. FORDYCE went out of the room, after having remained fifteen minutes in an heat of 130°; juſt before he left the room his pulſe made 139 beats in a minute, but the heat under his tongue, in his hand, and of his urine, did not exceed 100°. Here Dr. FORDYCE obſerves, that as there was no evaporation, but conſtantly a condenſation of vapour on his body, no cold was generated but by the animal powers. At the concluſion of this experiment, Dr. FORDYCE went into a room where the thermometer ſtood at 43°, dreſſed himſelf there, and immediately went out into the cold air, without feeling the leaſt inconvenience; on which he remarks, that the tranſition from very great heat to cold is not ſo hurtful as might be expected, becauſe the external circulation is ſo excited, as not to be readily overcome by the cold. Dr. FORDYCE has ſince had occaſion, in making other experiments, to go frequently into a much greater heat, where the air was dry, and to ſtay there a much longer time, without being affected nearly ſo much, for which he aſſigns two reaſons;

that dry air does not communicate its heat like air ſaturated with moiſture; and that the evaporation from the body, which takes place when the air is dry, aſſiſts its living powers in producing cold. It muſt be immediately perceived, that, beſides the principal object, theſe curious experiments throw great light on many other very important ſubjects of natural philoſophy.

January 23. The honourable Captain PHIPPS, Mr. BANKS, Dr. SOLANDER, and myſelf, attended Dr. FORDYCE to the heated chamber, which had ſerved for many of his experiments with dry air. We went in without taking off any of our cloaths. It was an oblong-ſquare room, fourteen feet by twelve in length and width, and eleven in height, heated by a round ſtove, or *cockle*, of caſt iron, which ſtood in the middle, with a tube for the ſmoke carried from it through one of the ſide walls. When we firſt entered the room, about 2 o'clock in the afternoon, the quickſilver in a thermometer which had been ſuſpended there ſtood above the 150th degree. By placing ſeveral thermometers in different parts of the room we afterwards found, that the heat was a little greater in ſome places than in others; but that the whole difference never exceeded 20°. We continued in the room above 20 minutes, in which time the heat had riſen about 12°, chiefly during the firſt part of our ſtay. Within an hour afterwards we went into this room again, without feeling any material difference, though the heat was conſiderably increaſed. Upon entering the room a third time, between five and ſix o'clock after dinner, we obſerved

ſerved the quickſilver in our only remaining thermometer at 198°(e): this great heat had ſo warped the ivory frames of our other thermometers that every one of them was broken. We now ſtaid in the room, all together, about 10 minutes; but finding that the thermometer ſunk very faſt, it was agreed, that for the future only one perſon ſhould go in at a time, and orders were given to raiſe the fire as much as poſſible. Soon afterwards Dr. SOLANDER entered the room alone, and ſaw the thermometer at 210°; but, during three minutes that he ſtaid there, it ſunk to 196°. Another time, he found it almoſt five minutes before the heat was leſſened from 210° to 196°. Mr. BANKS cloſed the whole, by going in when the thermometer ſtood above 211°; he remained ſeven minutes, in which time the quickſilver had ſunk to 198°; but cold air had been let into the room, by a perſon who went in and came out again during Mr. BANKS's ſtay. The air heated to theſe high degrees felt unpleaſantly hot, but was very bearable. Our moſt uneaſy feeling was a ſenſe of ſcorching on the face and legs; our legs particularly ſuffered very much, by being expoſed more fully than any other part to the body of the ſtove, heated red-hot by the fire within. Our reſpiration was not at all affected; it became neither quick nor laborious; the only difference was a want of that refreſhing ſenſation which accompanies a full inſpiration of cool air. Our time was ſo taken up with other obſervations that we did not count our pulſes by the watch: mine, to the beſt of my judgment by feeling it, beat at the rate of 100 pulſations in a minute, near the end of the firſt experiment; and Dr. SOLANDER's made 92 pulſations in a minute ſoon after we had gone out of the heated room. Mr. BANKS ſweated profuſely, but no one elſe; my ſhirt was only damp at the end of the experiment. But the moſt ſtriking effects proceeded from our power of preſerving our natural temperature. Being now in a ſituation in which our bodies bore a very different relation to the ſurrounding atmoſphere from that to which we had been accuſtomed, every moment preſented a new phænomenon. Whenever we breathed on a thermometer the quickſilver ſunk ſeveral degrees. Every expiration, particularly if made with any degree of violence, gave a very pleaſant impreſſion of coolneſs to our noſtrils, ſcorched juſt before by the hot air ruſhing againſt them when we inſpired. In the ſame manner our now cold breath agreeably cooled our fingers whenever it reached them. Upon touching my ſide, it felt cold like a corpſe; and yet the actual heat of my body, tried under my tongue, and by applying cloſely the thermometer to my ſkin, was 98°, about a degree higher than its ordinary temperature. When the heat of the air began to approach the higheſt degree which this apparatus was capable of producing, our bodies in the room prevented it from riſing any higher; and when it had been previouſly raiſed above that point, inevitably ſunk it. Every experiment furniſhed proofs of this: toward the end of the firſt, the thermometer was ſtationary: in the ſecond, it ſunk a little during the ſhort time we ſtaid in the room: in the third, it ſunk ſo faſt as to oblige us to determine that only one perſon ſhould go in at a time: and Mr. BANKS and Dr. SOLANDER each found, that his ſingle body was ſufficient to ſink the quickſilver very faſt, when the room was brought nearly to its *maximum* of heat.

(e) This thermometer ſtands, near the boiling point, about a degree too high; the ſcale is FAHRENHEIT's.

Theſe experiments, therefore, prove in the cleareſt manner, that the body has a power of deſtroying heat. To ſpeak juſtly on this ſubject, we muſt call it a power of deſtroying a certain degree of heat communicated with a certain quickneſs. Therefore in eſtimating the heat which we are capable of reſiſting, it is neceſſary to take into conſideration not only what degree of heat would be communicated to our bodies, if they poſſeſſed no reſiſting power, by the heated body, before the equilibrium of heat was effected; but alſo what time that heat would take in paſſing from the heated body into our bodies. In conſequence of this compound limitation of our reſiſting power, we bear very different degrees of heat in different mediums. The ſame perſon who felt no inconvenience from air heated to 211°, could not bear quickſilver at 120°, and could juſt bear rectified ſpirit of wine at 130°; that is, quickſilver heated to 120° furniſhed, in a given time, more heat for the living powers to deſtroy, than ſpirits heated to 130°, or air to 211°(*f*).

(*f*) Theſe numbers are the reſult of ſome experiments which were made on the firſt of February, in a room where the heat of the air was 65°. Mr. BANKS

And we had in the heated room where our experiments were made, a ſtriking though familiar inſtance of the ſame. All the pieces of metal there, even our watch-chains, felt ſo hot, that we could ſcarcely bear to touch them for a moment, whilſt the air, from which the metal had derived all its heat, was only unpleaſant. The ſlowneſs with which air communicates its heat was further ſhewn, in a remarkable manner, by the thermometers we brought with us into the room, none of which at the end of twenty minutes, in the firſt experiment, had acquired the real heat of the air by ſeveral degrees. It might be ſuppoſed, that by an action ſo very different from that to which we are accuſtomed, as deſtroying a large quantity of heat, inſtead of generating it, we muſt have been greatly diſordered. And indeed we experienced ſome inconvenience; our hands ſhook very much, and we felt a conſiderable degree of languor and debility; I had alſo a noiſe and giddineſs in my head. But it was only a ſmall part of our bodies that exerted the power of deſtroying heat with ſuch a violent effort as ſeems neceſſary at firſt ſight. Our cloaths, contrived to guard us from cold, guarded us from the heat on the ſame principles. Underneath we were ſurrounded with an atmo-

BANKS and I found that we could bear ſpirits which had been conſiderably heated and were now cooling, when the thermometer came to the 130th degree; cooling oil at 129°; cooling water at 123°; cooling quickſilver at 117°. And theſe points were pretty nicely determined; ſo that though we could bear water very well at 123°, we could not bear it at 125°, an experiment in which Dr. SOLANDER joined us. And our feelings with reſpect to all theſe points, ſeemed pretty exactly the ſame.

ſphere of air, cooled on one ſide to 98°, by being in contact with our bodies, and on the other ſide heated very ſlowly, becauſe woollen is ſuch a bad conductor of heat. Accordingly I found, toward the end of the firſt experiment, that a thermometer put under my cloaths, but not in contact with my ſkin, ſunk down to 110°. On this principle it was that the animals, ſubjected by M. TILLET to the intereſting experiments related in the Memoirs of the Academy of Sciences for the year 1764, bore the oven ſo much better when they were cloathed, than when they were put in bare: the heat actually applied to the greateſt part of their bodies was conſiderably leſs in the firſt caſe than in the laſt. As animals can deſtroy only a certain quantity of heat in a given time, ſo the time they can continue the full exertion of this deſtroying power ſeems to be alſo limited; which may be one reaſon why we can bear for a certain time, and much longer than can be neceſſary to fully heat the *cuticle*, a degree of heat which will at length prove intolerable. Probably both the power of deſtroying heat, and the time for which it can be exerted, may be increaſed, like moſt other faculties of the body, by frequent exerciſe. It might be partly on this principle that, in M. TILLET's experiments, the girls who had been uſed to attend the oven bore, for ten minutes, an heat which would raiſe FAHRENHEIT's thermometer to 280°: in our experiments, however, not one of us thought he ſuffered the greateſt degree of heat that he was able to ſupport.

A principal uſe of all theſe facts is, to explode the common theories of the generation of heat in animals. No attrition, no fermentation, or whatever elſe the mechanical and chemical phyſicians have deviſed, can explain a power capable of producing or deſtroying heat, juſt as the circumſtances of the ſituation require. A power of ſuch a nature, that it can only be referred to the principle of life itſelf, and probably exerciſed only in thoſe parts of our bodies in which life ſeems peculiarly to reſide. From theſe, with which no conſiderable portion of the animal body is left unprovided, the generated heat may be readily communicated to every particle of inanimate matter that enters into our compoſition. This power of generating heat ſeems to attend life very univerſally. Not to mention other well known experiments, Mr. HUNTER found a carp preſerve a coat of fluid water round him, long after all the reſt of the water in the veſſel had been congealed by a very ſtrong freezing mixture. And as for inſects, Dr. MARTINE (g) obſerved, that his thermometer, buried in the midſt of a ſwarm of bees, roſe to 97°. It ſeems extremely probable, that vegetables, together with the many other vital powers which they poſſeſs in common with animals, have ſomething of this property of generating heat. I doubt, if the ſudden melting of ſnow which falls upon graſs, whilſt that on the adjoining gravel walk continues ſo many hours unthawed, can be adequately explained on any other ſuppoſition. Moiſt dead ſticks are often found frozen quite hard, when in the ſame garden the tender growing twigs are not at all affected. And many herbaceous vegetables, of no great ſize, reſiſt every winter degrees of cold which are found ſufficient to freeze large bodies of water. It may be proper to add, that after each of the above mentioned experiments of bearing high degrees of heat, we went out immediately into the open air, without any precaution, and experienced from it no bad effect. The languor and ſhaking of our hands ſoon went off, and we have not ſince ſuffered the leaſt inconvenience.

(g) Eſſays Medical and Philoſophical, p. 331.

XIII. *The ſuppoſed Effect of boiling upon Water, in diſpoſing it to freeze more readily, aſcertained by Experiments.* *By* Joſeph Black, *M. D. Profeſſor of Chemiſtry at* Edinburgh, *in a Letter to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR, Edinburgh, Feb. 11, 1775.

Redde, Feb. 23, 1775.

WE had lately one day of a calm and clear froſt; and I immediately ſeized the opportunity, which I miſſed before, to make ſome experiments relative to the freezing of boiled water, in compariſon with that of water not boiled. I ordered ſome water to be boiled in the tea kettle four hours. I then filled with it a Florentine flaſk, and immediately applyed ſnow to the flaſk until I cooled it to 48° of FAHRENHEIT, the temperature of ſome unboiled water which ſtood in my ſtudy in a bottle; then putting four ounces of boiled, and four of the unboiled water, ſeparately, into two equal tea cups, I expoſed them on the outſide of a north window, where a thermometer pointed to 29°. The conſequence was, that ice appeared firſt upon the boiled water; and this, in ſeveral repetitions of the experiment, with the ſame boiled water, ſome of which were made nine hours after it was poured out of the tea kettle. The length of time which intervened between the firſt appearance of ice upon the two waters was different in the different experiments. One cauſe of this variety was plainly a variation of the temperature of the air, which became colder in the afternoon, and made the thermometer deſcend gradually to 25°. Another cauſe was the diſturbance of the water; when the unboiled water was diſturbed now and then by ſtirring it gently with a quill tooth-pick, the ice was formed upon it as ſoon, or very nearly as ſoon, as upon the other; and from what I ſaw, I have reaſon to think, that were it to be ſtirred inceſſantly, provided at the ſame time the experiment were made with quantities of water, not much larger or deeper than theſe, it would begin to freeze full as ſoon. In one of theſe trials, having inſpected my tea cups when they had been an hour expoſed, and finding ice upon the boiled water, and none upon the other, I gently ſtirred the unboiled water with my tooth-pick, and ſaw immediately, under my eye, fine feathers of ice formed on its ſurface, which quickly encreaſed in ſize and number, until there was as much ice in this cup as in the other, and all of it formed in one minute of time, or two at moſt. And in the reſt of the trials, though the congelation began in general later in the unboiled water than in the other; when it did begin in the former, the ice quickly encreaſed ſo as, in a very ſhort time, to equal, or nearly equal in quantity, that which had been formed more gradually in the boiled water. The opinion, therefore, which I have formed from what I have hitherto ſeen is, that the boiled and common water differ from one another in this reſpect; that whereas the common water, when expoſed in a ſtate of tranquillity to air that is a few degrees colder than the freezing point, may eaſily be cooled to the degree of ſuch air, and ſtill continue perfectly fluid, provided it ſtill remain undiſturbed: the boiled water, on the contrary, cannot be preſerved fluid in theſe circumſtances; but when cooled down to the freezing point, if we attempt to make it in the leaſt colder, a part of it is immediately changed into ice; after which, by the continued action of the cold air upon it, more ice is formed in it every moment, until the whole of it be gradually congealed before it can become as cold as the air that ſurrounds it. From this diſcovery it is eaſy to underſtand, why they find it neceſſary to boil the water in India, in order to obtain ice. The utmoſt intenſity of the cold which they can obtain by all the means they employ, is probably not greater than 31° or 30° of FAHRENHEIT's thermometer. Common water, left undiſturbed, will eaſily deſcend to this degree without freezing; and, if they have not the means of making it colder, may continue fluid for any time, provided it be not diſturbed: the refrigerating cauſes of that part of the world when they have done ſo much, have done their utmoſt, and can act no further upon the water. But this cannot happen to the boiled water; when the refrigerating cauſes have cooled it to 32°, the next effect they produce, is to occaſion in it the beginning of congelation, while the water is afterwards gradually aſſuming the form of ice, we know, by experience, that the temperature of it muſt remain at 32°; it cannot be made colder, ſo long as any conſiderable part of it remains unfrozen (a). The refrigerating cauſes continue, therefore, to have power over it, and to act upon it, and will gradually change the whole into ice, if their action be continued ſufficiently long.

The next object of inveſtigation may be the cauſe of this difference between the boiled and the common water. In conſidering this point, the following idea was ſuggeſted. As we know from experience, that by diſturbing common water, we haſten the beginning of its congelation, or render it incapable of being cooled below 32°, without being congealed; may not the only difference between it and boiling water, when they are expoſed together to a calm froſty air, conſiſt in this circumſtance: that the boiled water is neceſſarily ſubjected to the action of a diſturbing cauſe, during the whole time of its expoſure, which the other is not? One effect of boiling water long, is to expell the air which it naturally contains; as ſoon as it cools, it begins to attract and abſorb air again, until it hath recovered its former quantity; but this probably requires a conſiderable time. During the whole of this time, the air entering into it muſt occaſion an agitation or diſturbance in the water, which, though not ſenſible to the eye, may be very effectual in preventing it to become, in the leaſt, colder than the freezing point, without beginning to freeze, in conſequence of which its congelation muſt begin immediately after it is cooled to that point. When I reflect upon this idea, I remember a fact which appears to me to ſupport it ſtrongly. FAHRENHEIT was the firſt perſon who diſcovered that water, when preſerved in tranquillity, may be cooled ſome degrees below the freezing point without freezing. He made the diſcovery while he was endeavouring to obtain ice from water that had been purged of its air: with this intention he had put ſome water into little glaſs globes, and having purged it of air, by boiling and the air-pump, he ſuddenly ſealed up the globes, and then expoſed them to the froſty air. He was ſurprized to find the water remain unfrozen much longer than he expected, when at laſt he opened ſome of his globes, in order to apply a thermometer to the water, or otherwiſe examine what ſtate it was in. The immediate conſequence of the admiſſion of the air was a ſudden congelation which happened in the water; and in the reſt of his globes, a ſimilar production of ice was occaſioned by ſhaking them. The inference that may be drawn from theſe experiments of FAHRENHEIT's is ſufficiently obvious; it appears to me to remove all doubt with regard to the above ſuppoſition. Before theſe experiments of FAHRENHEIT occurred to my memory, I had planned a few, ſuggeſted by the above ſuppoſition, that might have led to the ſame concluſion; but the ſhort duration of the froſt, for one day only, did not give me time to put them in execution.

(a) Common water, when cooled in a ſtate of tranquillity to ſeveral degrees below the freeing point, will ſuddenly riſe up to it again, if diſturbed in ſuch a manner as to occaſion in it a beginning of congelation.

XIV.

EXPERIMENTS

ON THE

DIPPING NEEDLE,

MADE BY DESIRE OF THE

ROYAL SOCIETY.

BY

THOMAS HUTCHINS.

REDDE, FEBRUARY 16, 1775.

EXPERIMENTS ON THE DIPPING NEEDLE.

Stromneſs in the iſles of Orkney, lat. 58° 59′ North, long. 3° 30′ Weſt from London, June 9, 1774.

°	′	
75	50	The index placed Eaſt.
76	0	
75	45	
75	45	
75	40	
74	55	The index placed Weſt.
75	0	
75	20	
75	25	
75	25	The poles of the needle changed, the index placed Weſt.
76	10	
76	15	
76	25	The index facing the Eaſt.
77	0	

In theſe obſervations the needle was placed horizontal, and the vibration continued between nine and ten minutes. The inſtrument was ſet in the middle of a room up one pair of ſtairs; but being apprehenſive that the iron-grate, fender, poker, and tongs, might, in ſome meaſure, affect the needle, I determined to make a trial in the open air, and in a place free from ſuch obſtacles.

EXPERIMENTS ON THE DIPPING NEEDLE.

On the Holms in the entrance of Stromneſs Harbour, June 23, 1774. Variation *per* azimuth 24° Weſterly. Long. from London 3° 30′ Weſt, lat. 58° 59′ North.

°	′	
76	0	The index placed Weſt.
75	40	
75	45	
76	55	The index placed Eaſt.
76	10	
76	30	
74	45	The poles changed, and index Eaſt.
75	30	
75	0	
76	0	The index placed Weſt.
74	45	
75	0	

The needle in all theſe obſervations was left to vibrate from an horizontal poſition. The inſtrument was ſet on the top of the caſe (in which it was packed) and ſtood in the open air, in a fine ſunny day.

EXPERIMENTS ON THE DIPPING NEEDLE.

In Hudſon's Straits, July 23, 1774, lat. 62° 3′ North, long. 69° Weſt from London, variation 43° Weſterly.

°	′	
82	50	The Index placed Eaſt.
82	30	
82	40	
81	50	The index placed Weſt.
82	45	
83	45	
82	40	

The needle vibrated from an horizontal ſituation. Theſe obſervations were made on a large piece of ice, to which the three ſhips were grappled. I imagine the firſt four experiments may be depended on, as it was calm weather; but afterwards, a breeze ſpringing up gave the ice a circular motion, which made it impoſſible to keep the inſtrument exactly in the magnetic meridian, as may be ſeen in the fifth and ſixth experiments. I re-adjuſted the inſtrument for the laſt obſervation; but finding the ice ſtill continued in motion, I judged it unneceſſary to make any farther experiments at this time, as it could not be done with that exactneſs I could wiſh, or give ſatisfaction to the learned Society to whom I have the honour of tranſmitting theſe remarks.

EXPERIMENTS ON THE DIPPING NEEDLE.

In Hudſon's Straits, July 27, 1774, lat. 62° 23′ North, long. 71° 30′ Weſt from London, variation 42° 50′ Weſterly *per* azimuth.

°	′	
81	45	The index placed Eaſt.
83	12	
82	12	
83	0	The index placed Weſt.
82	45	
83	45	
83	30	The poles changed, and index Weſt.
84	0	
83	35	
85	0	The index placed Eaſt.
83	25	
83	45	

I met with the ſame impediment as in the laſt trial of the inſtrument, the field of ice turning round, ſo as to remove the needle conſtantly out of the magnetic meridian; however, I endeavoured to be exact, and re-adjuſted the poſition of the inſtrument twice during the obſervations.

EXPERIMENTS ON THE DIPPING NEEDLE.

In Hudſon's Straits, July 28, 1774, lat. 62° 25′ North, long. 71° 30′ Weſt from London, variation *per* azimuth 44° Weſt.

°	′	
83	0	The index pointing Weſt.
83	0	
83	30	The index pointing Eaſt.
83	0	
81	30	The poles changed, and index Eaſt.
81	40	
82	8	The index pointing Weſt.
82	0	

Theſe obſervations were made with the aſſiſtance of Captain Richards, on a table in the cabbin of the Prince Rupert. We uſed all imaginary care to render them exact; yet, at the concluſion, I found the ſhip, though faſt to a field of ice, had altered the poſition of her head: for by placing the index to the North line of the inſtrument, the needle ſtood at 88° 10′ Weſt, inſtead of being perpendicular at 90°.

EXPERIMENTS ON THE DIPPING NEEDLE.

In Hudſon's Bay, Auguſt 14, 1774, lat. 56° 53′ North, long. 85° 22′ Weſt from London, variation *per* azimuth 24° Weſt.

°	′	
82	0	The index placed Eaſt.
82	0	
82	15	The index placed Weſt.
82	20	
82	50	The poles changed, and index Eaſt
82	35	
82	30	The index placed Eaſt.
82	15	

Theſe experiments were made in conjunction with Captain Richards, in the cabbin of the Prince Rupert, whilſt ſhe lay amongſt ice. The ſhip frequently varied the poſition of her head a point of the compaſs; but by replacing the inſtrument as often as we found occaſion, I have the greateſt reaſon to think theſe obſervations (which took up above three hours) are pretty accurate.

EXPERIMENTS ON THE DIPPING NEEDLE.

At Mooſe Fort in Hudſon's Bay, September 8, 1774, lat. 51° 20′ North, long. 82° 30′ Weſt from London, variation 17° Weſt.

80°	25′	The index placed Weſt.
80	15	
80	35	
79	0	
80	30	The Index placed Eaſt.
81	25	
80	13	
81	13	
79	10	The poles reverſed, index Eaſt.
80	45	
79	50	
79	10	
79	10	The index placed Weſt.
80	25	
79	45	
80	5	

The obſervations were made on ſhore. So remarkable difference between them, when I was expecting quite the reverſe, ſurprized me as much as the increaſed inclination of the needle from obſervations made nearly in the ſame parallel of latitude in London. I endeavoured, by drawing a magnetical meridional line with chalk, and paying the greateſt attention to keeping the inſtrument perfectly ſteady and horizontal, to render theſe experiments accurate, and fulfil the intention of the Royal Society.

RXPERIMENTS ON THE DIPPING NEEDLE.

At Albany Fort in Hudſon's Bay, September 14, 1774, long. 82° 30′ Weſt, lat. 52° 22′ North, variation 17° Weſt.

°	′
80	13
80	25
79	37
79	55

I made a trial of the inſtrument at this place, but having loſt the ſlip of paper on which I had noted the experiments, I was dubious whether I ſhould inſert the above or not. I can only recollect theſe four, and am not poſitive which way the index ſtood; however, I remember that the mean of all the obſervations I made was ſomething leſs than 80°. Time will not permit me to repeat the operation during the ſhip's ſtay in theſe parts; I muſt therefore defer it to a future period. During the winter, I ſhall have frequent opportunities of amuſing myſelf this way; and the reſpect I bear the Royal Society, makes every ſervice I render to that illuſtrious body an additional happineſs to

Their devoted ſervant,

Albany Fort,
September 17, 1774.

THOMAS HUTCHINS.

OBSERVATIONS ON HOY, 1774.

Month.	Hour.	Barometer.	Thermometer.	Weather.	Circumſtances.
1774. June 11.	0 15	28,63	59	Clear.	On the top of the hill.
	0 30	28,60	56½	Foggy.	Ditto.
	4 15	30,22	63	Clear.	At low water mark.

Hoy is a remarkable high hill near Stromneſs, in the Orkneys, and is placed by Mr. MACKENZIE in lat. 58° 58′ North, and long. 3° 30′ Weſt from London. The two firſt obſervations were made on the higheſt part of the hill. Soon after the firſt, a fog was ſeen below ariſing from the water, at length it reached the ſummit of the hill; the air ſeemed very raw and cold to the touch, and the inſtruments ſhewed as in the ſecond obſervation. The barometer continued at 28,60 inches after the fog was gone off, but the thermometer roſe two or three degrees. The laſt obſervation was made at low water mark, about half a mile from the bottom of the hill. THOMAS HUTCHINS.

"*The height of* Hoy *above low-water mark, according to theſe obſervations ſhould* "*be* 249,93 *fathoms, or as near as may be* 500 *yards, neglecting the correction for* "*the difference that may be ſuppoſed in the temperature of the quickſilver at the two* "*ſtations, the quantity of which is uncertain.*" S. HORSLEY.

A

METEOROLOGICAL JOURNAL

FOR THE YEAR 1774.

KEPT

AT THE ROYAL SOCIETY'S HOUSE

BY ORDER OF

THE PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for 1774.

		Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
		H.	M.			Inches.	Inch.	Points.	Str.	
Jan.	1	8	0	26,0	36,0	29,57		W	1	Froſt.
		2	30	31,5	36,5	29,55				
	2	8	30	30,8	34,5	29,44		NW	1	Fair.
	3	8	15	26,0	32,5	29,765		NNW	1	Fair.
		2	10	32,5	33,0	29,89				
	4	8	40	24,0	31,0	30,015		SW	1	Fine.
		2	0	32,0	32,0	29,995		WSW	1	
	5	8	15	25,8	32,0	30,135		SW	1	Fog.
		4	0	30,0	32,0	30,175		SW	1	Fog.
	6	8	5	34,7	33,0	29,975		SW	1	Rain.
		2	10	41,0	35,5	29,925		SSW	1	Rain.
	7	8	5	35,7	36,5	29,585	0,596	NNW	2	Clear.
		2	0	39,0	38,5	29,67		NW	1	
	8	8	15	28,0	36,0	29,93		SSW	1	Fog.
		3	15	36,0	37,0	29,84		E		
	9	8	15	37,5	38,0	29,475	0,956	N	1	Rain.
		2	0	33,0	38,5	29,615		N	1	Snow.
	10	8	0	30,5	36,5	29,815	0,340	E	1	Clear.
		2	15	31,0	36,5	29,835				
	11	8	0	27,0	33,5	29,735		ENE	1	Fine.
		3	0	32,0	34,0	29,70		ENE		
	12	8	15	28,0	32,0	29,67		ENE	1	Fair.
		2	0	32,5	33,5	29,64		ENE	1	
	13	8	0	30,0	32,0	29,44		E by N	1	Fog.
		2	0	34,0	34,0	29,325				
	14	8	10	42,0	38,0	28,79	0,184	SW	2	Rain.
		2	10	46,0	40,5	28,845		SW		Rain.
	15	8	0	41,5	41,5	29,20	0,042	SW	1	Cloudy.
		2	0	44,7	43,0	29,33		NW	1	
	16	8	15	45,5	44,0	29,265	0,098	SW	1	Cloudy.
		2	10	50,0	45,0	29,22		SW		Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	48,5	46,0	29,27	0,010	SW	2	Cloudy, windy night.
	2	0	50,5	47,5	28,935		SSW	2	Rain.
18	8	0	34,5	45,0	29,11	0,210	NNE	2	Snow, windy night.
	2	0	35,0	50,5	29,34		NE		
19	8	0	27,0	39,0	29,895		W by N	2	Fair, windy night.
	2	10	34,0	39,0	29,92		NW	1	Fair.
20	8	0	29,5	35,5	29,75		E by S	1	Fog.
	2	0	33,0	36,0	29,65		E by S	1	Fair.
21	8	0	30,0	34,5	29,73		E	1	Fair.
	2	5	35,0	35,5	29,83		E	1	Fair.
22	8	0	27,5	33,5	29,76		E by S	1	Fair.
	2	0	36,0	42,0	29,56		SE	1	
23	8	0	40,5	38,5	28,99	0,136	SW	2	Cloudy.
	3	0	41,0	39,0	29,02		SW	1	Fair.
24	8	0	35,0	38,0	28,85		SW	1	Snow.
	3	0	39,0	39,0	28,98		SW	1	Cloudy.
25	8	0	30,5	37,0	29,455		WSW	1	Fair.
	2	0	36,0	38,0	29,53		WSW	1	Fair.
26	8	0	32,5	37,0	29,35	0,058	SW	1	Fair.
	2	30	39,5	39,0	29,46		WSW	1	Fair.
27	8	0	42,0	39,0	29,50	0,117	SW	1	Rain.
	3	0	48,0	41,5	29,34		SW	1	Rain.
28	8	0	36,0	42,0	29,51	0,131	SW	1	Fair.
	2	10	41,0	43,5	29,56		SW	1	Fair.
29	8	0	37,5	41,5	29,58	0,058	SE	1	Rain.
	3	0	42,5	42,0	29,57		W	1	Fog.
30	8	0	31,5	40,0	29,88	0,022	W	1	Fair.
	2	15	41,0	41,0	29,94		NW	1	Fine.
31	8	0	27,0	37,0	30,055		N by E	1	Fair.
	2	0	35,0	38,0	30,07		N	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	31,0	36,0	30,20		NE	1	Fair.
	2	0	32,0	36,0	29,87		SW	1	Snow.
2	8	0	24,5	33,0	30,225		NE	1	Fair.
	2	0	33,5	35,0	30,28		NE	1	Fair.
3	8	0	29,0	33,0	30,355		NNE	1	Fair.
	2	0	37,0	36,0	30,36		NE	1	Thaw.
4	8	30	35,5	36,5	30,435	0,114	NE	1	Fog.
	2	10	40,0	38,0	30,46		NNE	1	Thaw.
5	8	0	29,0	36,0	30,44		E	1	Fair.
	2	0	40,0	38,0	30,44		N	1	Fine.
6	8	0	27,5	35,0	30,31		W	1	Fair.
	2	10	36,5	36,5	30,27		WSW	1	Fine.
7	8	0	36,0	36,0	30,04		WSW	1	Fog.
	2	0	46,0	39,0	29,95		SWbyW	2	Fair.
8	8	0	41,0	40,0	29,25	0,211	W	2	Fair.
	2	0	34,0	39,0	29,55		NW	2	Fine.
9	8	0	25,0	35,0	29,92		W	1	Fair.
	3	15	34,0	36,0	29,91		NW	1	Fine.
10	8	0	32,0	35,0	30,00		WSW	1	Fog.
	2	0	38,5	36,5	29,97		S	1	Rain.
11	8	0	45,0	40,0	29,41	0,330	SW	1	Rain, windy night.
	2	0	49,0	42,0	29,30		SW	1	Fine.
12	8	0	40,0	41,5	29,58	0,102	SW	1	Fair.
	3	15	48,5	44,0	29,74		W	1	Fair.
13	8	0	43,5	45,5	29,81	0,040	SW	2	Fair, windy night.
	2	0	51,0	47,0	29,87		SW	1	Fair.
14	8	0	47,0	48,0	29,84		SSW	1	Fair, windy night.
	2	10	52,0	50,0	29,74		SSW	1	Fair.
15	8	0	45,0	48,0	29,74	0,185	SSW	1	Fair.
	2	0	50,0	54,0	29,56		SSW	2	Fair.
16	8	0	48,5	49,0	29,19	0,079	SSW	2	Rain, windy night.
	2	5	47,5	50,0	29,20		SW	2	Rain.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	40,0	47,0	29,43	0,057	SW	2	Fair.
	2	0	44,5	47,5	29,55		WSW	2	Fine.
18	8	0	36,0	44,5	29,88		SW	1	Fair.
	2	40	48,5	46,5	29,93		W	1	Fair.
19	8	0	40,5	44,5	30,09		SSW	1	Fair.
	2	0	47,5	46,0	29,96		S	1	Fair.
20	8	0	38,5	45,0	29,97	0,200	SW	1	Fair.
	2	0	48,0	47,0	30,04		SW	1	Fine.
21	8	0	43,0	45,5	29,66		S	2	Fair, windy night.
	2	15	45,0	47,0	29,45		WSW	2	Rain.
22	8	0	39,0	44,0	29,90	0,195	SSW	1	Rain.
	2	0	46,0	45,0	29,61		SSW	2	Rain.
23	8	0	45,0	46,0	29,47	0,127	SW	1	Fair, windy night.
	2	40	50,5	48,0	29,39		SSW	1	Rain.
24	8	0	40,5	47,5	29,16	0,170	SW	3	Fair, windy night.
	2	0	49,0	49,0	29,30		WSW	3	Fair.
25	8	0	39,5	46,5	29,26	0,090	E	1	Rain, windy night.
	2	0	40,5	46,0	28,95		N	1	Rain.
26	8	0	34,5	42,0	29,55	0,280	W by N	1	Fair.
	2	0	41,0	43,0	29,68		W by N	1	Fine.
27	8	0	33,0	40,0	30,07		WSW	1	Fair.
	2	0	44,5	41,5	30,15		W	1	Fine.
28	8	0	44,5	44,0	29,80	0,180	SW	2	Rain, windy night.
	2	0	52,0	45,5	29,68		SW	2	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 1	8	0	37,0	43,5	29,85	0,043	WSW	1	Fair.
	2	0	46,0	46,0	29,93		W	2	Fair.
2	8	0	37,5	42,5	29,57		SW	1	Fair.
	2	0	40,0	43,5	29,46		WSW	1	Fair.
3	8	0	33,5	40,5	29,56	0,083	W	1	Fair.
	2	0	43,5	43,0	29,615		NW	1	Fair.
4	8	0	37,5	43,5	29,33	0,020	SE	1	Rain.
	2	20	49,0	46,0	29,16		SW	1	Fair.
5	8	0	41,0	44,0	29,155	0,127	SW	1	Fair.
	2	0	45,5	45,0	29,14		SW	1	Rain.
6	8	0	44,0	45,0	29,34	0,410	SW	1	Fair.
	2	40	54,5	48,0	29,44		SW	1	Fair.
7	7	30	50,0	48,5	29,54	0,142	SW	2	Rain, windy night.
	2	0	51,0	51,0	29,61		SW	2	Rain.
8	7	30	52,0	51,5	29,60	0,328	SW	2	Rain.
	2	0	47,5	52,0	29,73		N	2	Rain.
9	7	30	39,5	47,5	29,94	0,220	NE	2	Rain.
	2	40	40,0	46,0	29,81		NE	2	Rain.
10	7	30	35,0	41,5	29,90	0,387	NE	2	Fair.
	2	0	39,0	48,0	29,89		NE	2	Fine.
11	7	30	34,0	41,5	29,87		NE	2	Fair.
	2	15	42,0	43,5	29,81		NE	2	Fair.
12	8	0	30,0	39,5	29,74		NE	1	Fair.
	2	0	44,0	42,0	29,74		NE	2	Fine.
13	8	0	36,0	40,0	29,97		ENE	1	Fair.
	2	0	41,0	41,5	30,01		E	1	Fine.
14	7	35	31,5	39,0	30,06		NE	1	Fair.
15	7	30	31,5	38,0	29,93		NE	1	Fair.
	2	0	47,0	46,0	29,88		ESE	1	Fine.
16	7	35	32,0	40,0	29,765		ENE	1	Fog.
	2	0	50,5	50,0	29,69		ENE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 17	7	30	37,5	43,0	29,62		ENE	1	Fair.
	2	0	60,0	47,0	29,61		SSW	1	Fine.
18	7	30	48,0	49,0	29,55		S	2	Fair.
	2	0	56,5	52,0	29,52		SSE	1	Fine.
19	7	30	45,5	49,5	29,49	0,010	SE	1	Fair.
	2	0	57,5	54,0	29,55		SE	1	Fine.
20	7	35	45,0	49,5	29,73		NE	1	Fair.
	2	0	55,5	53,5	29,78		E	1	Fine.
21	7	35	41,5	49,5	30,01		NE	1	Fair.
	2	0	54,0	52,0	30,09		N	1	Fine.
22	7	30	40,5	49,0	30,30		NE	1	Fair.
	2	0	50,0	51,0	30,31		NNE	1	Fine.
23	7	40	42,0	48,0	30,30		N	1	Fair.
	2	0	46,5	49,0	30,30		NNE	1	Fair.
24	7	30	41,5	47,5	30,33	0,010	E by N	1	Fair.
	2	15	54,0	57,0	30,33		NE	1	Fine.
25	7	0	37,5	47,5	30,22		NNE	1	Fog.
	2	30	53,0	50,0	30,12		NE	1	Fine.
26	7	25	37,5	47,0	30,065		S	1	Fair.
	2	0	56,5	50,0	30,05		WSW	1	Fine.
27	7	40	41,0	48,0	30,07		SSW	1	Fair.
	2	0	59,0	52,0	30,01		S	1	Fine.
28	7	20	46,0	50,0	29,94		NE	1	Fine.
	2	15	60,0	61,5	29,99		NE	1	Fine.
29	7	25	43,0	44,0	30,10		NE by N	1	Fine.
	2	0	57,0	52,5	30,09		NE by N	1	Fine.
30	7	30	41,5	52,0	30,05		N	1	Fine.
	1	45	55,0	56,0	29,98		SW by S	1	Fine.
31	7	30	38,0	49,0	29,90		NE	0	Fog.
	2	0	59,0	56,0	29,725		SE	0	Fine.

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	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	S.r.	
Apr. 1	7 20	44,0	50,0	29,65		E	1	Fine.
	2 0	54,0	52,0	29,58		E	2	Fine.
2	7 0	42,0	48,5	29,41		E	1	Fine.
	2 0	53,0	51,0	29,48		E	1	Fine.
3	7 15	46,5	48,5	29,35		S	1	Fine.
	2 0	54,0	52,0	29,33		SSE	2	Fair.
4	7 0	43,0	47,0	29,50	0,415	SW	2	Fine.
	2 0	53,0	52,0	29,68		SW	1	Fine.
5	7 0	42,5	48,0	29,78		SSW	1	Rain.
	2 0	52,5	50,5	29,765		SSW	2	Fine.
6	7 0	43,5	48,0	29,575	0,122	S	2	Rain.
	2 0	50,0	50,0	29,37		S	2	Rain.
7	7 0	44,0	48,5	29,35	0,188	S by W	1	Fair.
	2 0	53,0	51,0	29,36		S	2	Rain.
8	7 0	43,0	48,5	29,39	0,130	S	1	Fair.
	2 15	51,5	51,0	29,45		SSW	1	Fair.
9	7 0	36,5	47,0	29,56		NE	1	Fog.
	2 0	52,0	49,5	29,56		N	1	Fair.
10	7 10	44,0	48,5	29,625		W	1	Fair.
	2 0	60,0	51,5	29,69		SSW	1	Fine.
11	7 0	48,0	50,5	29,805		S	1	Fair.
	2 0	57,5	53,5	29,90		SE	1	Fair.
12	7 0	51,0	53,5	30,05		SSE	1	Fine.
	2 0	65,0	58,0	30,105		SE	1	Fine.
13	7 0	46,0	53,0	30,185		N	1	Fine.
	2 0	62,0	58,0	30,21		ENE	2	Fine.
14	7 0	45,5	51,5	30,24		NE	2	Fair.
	2 0	51,0	53,5	30,21		NE	1	Fine.
15	7 0	45,0	50,0	29,98		NE	1	Fair.
	2 0	48,5	51,0	29,88		NE	2	Fair.
16	7 0	42,5	49,0	29,82		NE	1	Fair.
	2 0	48,5	50,0	29,79		N by E	1	Fair.

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	Time		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points	Str.	
Apr. 17	7	25	42 0	48,0	29,875		SW	1	Fair.
	2	0	53,0	50,0	29,88		WSW	1	Fair.
18	7	0	47,5	50,0	29,63	0,082	SW	1	Fair.
	2	15	51,5	52,5	29,58		WNW	1	Fair.
19	7	0	38,0	46,0	29,80	0,065	NW	2	Fine.
	2	0	49,0	48,0	29,785		W	2	Fair.
20	7	0	42,0	46.5	29,74		NW	1	Fair.
	2	0	50,0	48,5	29,81		NNE	1	Fine.
21	7	0	38,0	45,0	30,08		W	1	Fine.
	3	0	54,0	49,0	30,08		W	1	Fine.
22	7	0	50,0	50,0	30,04		WSW	1	Fine.
	2	0	57,0	52,5	30,06		WSW	1	Fair.
23	7	0	51,5	53,0	30,11		WSW	1	Fine.
	2	0	67,0	58,0	30,17		WSW	1	Fine.
24	7	30	54,0	55,5	30,175		W	1	Fair.
	2	0	65,5	59,0	30,17		W by S	2	Fair.
25	7	0	51,0	57,5	30,08	0,005	SW	1	Rain.
	2	0	65,0	60,0	29,875		SW	1	Fine.
26	7	0	46,0	56,0	29,65	0,035	W	2	Fine.
	2	0	55,0	56,0	29,615		W	2	Fine.
27	7	0	39,0	50,0	29,605		W	1	Fine.
	2	0	53,0	52,0	29,62		W	1	Fine.
28	7	0	44,5	49,5	29,71		N	1	Fair.
	2	0	43,5	49,0	29,71		N	2	Rain.
29	7	0	46,5	49,5	29,86	0,200	NW	1	Rain.
	2	15	55,5	51,5	29,90		N by W	1	Fair.
30	7	0	50,	53,0	29,925		N	1	Fair.
	2	0	58,5	55,5	29,98		NE	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 1	7	0	46,0	52,5	30,005		NE	2	Fair.
	2	0	58,0	55,0	29,97		NE	2	Fine.
2	7	0	50,0	53,0	29,58	0,020	NE	1	Cloudy.
	2	30	60,0	56,0	29,43		SSW	2	Cloudy.
3	7	10	51,0	55,0	29,38		NW	1	Cloudy.
	2	0	57,0	56,5	29,46		WSW	1	Rainy.
4	7	0	49,0	54,0	29,635		NE	1	Fine.
	2	6	63,0	60,0	29,65		ENE	2	Fine.
5	7	0	45,0	52,0	29,52	0,140	NE	1	Rainy.
6	7	0	48,0	53,5	29,53	0,043	NE	1	Fine.
	2	0	54,5	55,0	29,58		E	1	Fair.
7	7	0	46,0	51,5	29,84	0,363	NW	1	Fair.
	2	0	56,0	53,5	29,97		W by N	1	Fine.
8	7	30	47,5	51,5	30,13		ENE	1	Fair.
	2	0	63,0	55,0	30,11		SSE	1	Fine.
9	7	0	52,0	54,5	30,105	0,042	E	1	Fair.
	2	0	66,5	58,0	30,09		E	1	Fine.
10	7	0	55,5	56,5	29,96		NE	1	Fair.
	2	0	68,0	60,5	29,91		ENE	1	Fine.
11	7	0	53,0	58,0	29,85	0,048	ENE	2	Rain.
	2	0	60,0	60,5	29,89		NE	1	Fine.
12	7	0	52,5	57,5	29,97	0,088	N	1	Fair.
	2	0	65,5	61,0	30,01		N	1	Fine.
13	7	0	48,0	57,0	30,12		N	1	Fair.
	2	0	64,0	60,0	30,13		N	1	Fine.
14	7	0	52,0	57,5	30,12		NE	1	Fine.
	2	0	69,0	61,5	30,12		NE	1	Fine.
15	7	20	53,0	57,5	30,11		NE	1	Fine.
	2	0	66,5	60,5	30,11		NE	1	Fine.
16	7	0	46,5	56,0	30,175		NE	2	Fine.
	2	15	56,5	57,0	30,175		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 17	7	0	48,0	54,0	30,12		NE	1	Fair.
	2	0	52,5	55,0	30,15		NE	1	Fine.
18	7	0	45,0	51,5	30,15		SSE	1	Fine.
	2	0	50,5	51,5	30,15		NE	1	Fair.
19	7	0	47,0	51,0	30,155		SW	1	Fair.
	2	0	61,0	55,0	30,15		N by W	1	Fine.
20	7	0	52,0	54,5	30,15		E	1	Fine.
	2	0	63,0	57,5	30,09		E	1	Fine.
21	7	0	54,0	55,5	29,75		E	1	Fair.
	2	0	59,5	57,5	29,61		E	1	Fair.
22	7	10	58,5	59,0	29,35		SSW	1	Fine.
	2	0	65,0	61,0	29,34		SSE	1	Fair.
23	7	0	53,0	58,5	29,34		S	1	Fair.
	2	0	54,5	59,0	29,34		S by E	1	Rain
24	7	0	53,5	57,0	29,52	0,156	S by E	1	Rain.
	2	0	64,0	59,5	29,55		SSW	1	Fair.
25	7	0	50,5	57,0	29,74	0,121	NW	1	Fair.
	2	0	58,0	59,0	29,73		NW	1	Cloudy.
26	7	0	47,0	55,0	29,925	0,093	N	2	Cloudy.
	2	0	53,0	55,5	29,99		N	2	Fair.
27	7	0	45,0	51,0	29,96	0,076	NNW	2	Cloudy.
	2	0	56,0	53,5	29,99		NNW	2	Fair.
28	7	0	47,0	51,5	30,05	0,064	NW	1	Fine.
	1	45	55,0	54,0	30,05		NNW	1	Fair.
29	7	15	48,0	52,5	30,02	0,059	NW	1	Cloudy.
	2	0	50,5	53,5	29,92		NW	1	Rain.
30	7	0	47,0	51,5	29,79	0,050	SW	1	Cloudy.
	2	0	61,5	55,0	29,80		SW	1	Fine.
31	7	0	51,5	54,0	29,81	0,050	S by E	1	Rain.
	2	0	66,5	61,0	29,81		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 1	7	0	55,5	59,0	29,93	0,050	SSW	1	Fine.
	2	0	69,5	63,0	29,95		SSE	2	Fine.
2	7	0	63,0	62,0	29,93	0,046	S	2	Fair.
	2	0	66,0	63,5	29,92		S	2	Fine.
3	7	0	56,0	61,5	29,97	0,854	SW	1	Fine.
	2	15	66,0	63,5	29,88		SW	1	Fair.
4	7	0	52,0	60,0	29,65		WNW	1	Rain.
	4	30	54,5	51,0	29,65		NNE	1	Rain.
5	7	0	57,0	60,0	29,69	0,204	WNW	1	Fine.
	2	0	62,5	62,0	29,74		NW	1	Fine.
6	7	0	56,0	60,5	29,68	0,130	S	1	Fair.
	2	0	67,0	63,5	29,68		E	1	Fine.
7	7	0	54,5	61,0	30,10		NNW	1	Fine.
	2	0	72,0	64,0	30,17		NNW	1	Fine.
8	7	10	57,0	62,0	30,34		SE	0	Cloudy.
	2	0	68,5	64,5	30,34		NW	1	Fine.
9	7	0	65,0	65,0	30,255		W	1	Fair.
	2	0	75,5	68,5	30,24		W	1	Fine.
10	7	0	61,0	66,0	30,21		NE	0	Fine.
	2	0	64,0	67,0	30,18		E	0	Fair.
11	7	0	61,5	64,0	30,04		SW	0	Fair.
	2	0	71,0	67,5	29,95		E	1	Fair.
12	7	0	56,0	62,0	29,75		E	1	Fair.
	2	0	65,0	65,0	29,66		E	1	Cloudy.
13	7	0	59,5	64,5	29,60		N	2	Fine.
	2	0	70,0	66,0	29,74		WNW	2	Fine.
14	7	0	58,0	64,0	29,96		SW	1	Fair.
	2	0	72,0	67,0	29,97		SW	2	Fine.
15	7	0	66,5	67,0	29,85		S	1	Fair.
	2	0	77,0	70,0	29,84		SW	1	Fine.
16	7	0	62,0	68,0	29,96		S	1	Fine.
	2	0	74,5	69,5	29,99		SW	1	Fine.

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	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
June 17	7 0	63,0	66,0	29,99	0,060	SE	1	Rain.
	2 0	74,5	71,0	29,93		W	1	Fair.
18	7 0	57,0	65,0	30,01	0,170	SW	1	Fair.
	2 0	70,5	68,5	30,07		W	1	Fine.
19	7 30	60,5	66,0	29,90		SE	1	Fine.
	2 0	74,5	69,0	29,84		S	1	Fine.
20	7 0	60,0	65,5	29,475	0,019	S by W	1	Rain.
	2 0	65,0	67,0	29,47		S by W	2	Cloudy.
21	7 0	56,0	63,5	29,54	0,050	SW	1	Cloudy.
	2 10	61,5	64,0	29,64	0,510	S	1	Rain.
22	7 0	54,0	60,0	29,85		NNE	1	Cloudy.
	2 0	59,5	62,0	29,90		NW	1	Cloudy.
23	7 0	54,0	61,0	29,94	0,037	W	1	Cloudy.
	2 0	61,0	61,5	29,98		SW	1	Cloudy.
24	7 0	60,0	61,5	29,92		WSW	1	Fair.
	2 0	69,0	64,0	29,97		SW	1	Fair.
25	7 0	61,0	63,5	30,03		SSW	1	Fair.
	2 0	72,0	67,0	30,00		SSW	1	Fair.
26	7 0	67,0	66,5	29,92		SW	1	Fine.
	2 0	77,5	70,0	29,92		SW	1	Fine.
27	7 0	62,5	68,5	29,90		SW	1	Fine.
	2 0	69,0	69,5	29,87		SW	1	Fair.
28	7 0	60,5	66,5	29,78	0,026	SSW	1	Cloudy.
	2 30	69	67,5	29,80		SW	2	Fair.
29	7 0	59,5	65,5	29,93	0,028	WSW	2	Fine.
	2 10	67,0	65,0	29,94		SW	2	Fine.
30	7 0	58,5	62,5	29,83	0,089	SW	2	Cloudy.
	2 10	67,0	65,0	29,88		SSW	2	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	59,0	63,0	30,00		SW	1	Cloudy.
	2	0	73,5	66,5	30,01		SW	1	Fine.
2	7	0	62,0	65,5	30,10		S	1	Fair.
	2	0	76,0	69,0	29,92		SW	1	Fine.
3	7	0	61,0	67,5	29,71		S	1	Cloudy.
	2	0	75,5	70,5	29,74		SW	2	Fine.
4	7	0	60,0	67,0	29,84		SW	1	Fine.
	2	0	68,5	68,5	29,88		SW	2	Fine.
5	7	0	59,0	65,0	29,98	0,045	SSW	1	Fair.
	2	0	68,0	67,0	29,92		SSW	2	Fair.
6	7	0	58,0	65,0	29,77	0,178	WSW	1	Fine.
	2	0	63,0	65,0	29,75		WSW	2	Fine.
7	7	0	56,0	62,0	30,04	0,208	W	1	Fair.
	2	0	72,5	66,5	30,05		WSW	1	Fine.
8	7	0	64,0	63,0	29,93		SSE	1	Fine.
	2	30	68,5	66,0	29,92		SSE	1	Fair.
9	7	0	57,5	64,5	29,98	0,060	SW	1	Fine.
	2	0	64,5	66,5	29,99		SW	2	Rain.
10	7	20	59,0	63,0	29,99	0,226	SSW	1	Fine.
	2	5	69,0	65,0	29,92		SSW	1	Fine.
11	7	0	59,5	64,0	29,96	0,062	SW	1	Fair.
	2	0	60,5	64,0	29,99		NW	1	Rain.
12	7	0	58,0	63,0	30,03	0,130	SW	1	Fair.
	2	0	67,5	65,0	29,98		SW	2	Fine.
13	7	0	61,0	64,0	29,77	0,053	WSW	1	Fair.
	2	0	68,5	66,0	29,90		WNW	1	Fair.
14	7	0	61,0	64,0	29,61	0,282	SSW	1	Cloudy.
	2	10	63,0	65,0	29,80		NW	2	Fine.
15	7	0	58,5	63,0	30,08	0,039	NW	2	Fine.
	2	0	69,0	65,0	30,19		NW	2	Fine.
16	7	0	56,5	62,0	30,32		NW	1	Fine.
	2	5	71,0	65,5	30,30		NW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	20	60,0	65,0	30,235		WSW	1	Fine.
	2	20	68,5	67,0	30,21		SW	1	Fair.
18	7	0	60,0	65,0	30,19		WNW	1	Fair.
	2	0	68,0	67,0	30,21		WNW	1	Fair.
19	7	0	59,5	63,5	30,16		SW	1	Fair.
	2	0	66,0	65,5	30,05		WSW	2	Fair.
20	7	0	58,5	61,5	29,87	0,168	WNW	1	Fine.
	2	0	64,0	64,5	29,92		NW	1	Fine.
21	7	0	55,5	60,0	30,03	0,063	SW	1	Fair.
	2	0	70,5	64,5	30,03		SW	1	Fair.
22	7	0	57,0	62,5	30,26	0,102	N	1	Fair.
	2	0	67,5	65,0	30,33		N by E	1	Fine.
23	7	0	63,0	65,0	30,36		WSW	1	Fair.
	2	0	77,5	68,0	30,33		S by W	1	Fine.
24	7	0	60,0	66,5	30,22		S	0	Fine.
	2	0	77,5	69,0	30,14		S by W	0	Fine.
25	7	0	63,0	68,5	30,07		S	0	Fair.
	2	0	81,0	72,0	30,05		S by E	0	Fine.
26	7	0	63,0	70,0	29,92		NE	1	Fine.
	2	0	83,5	73,5	29,78		E by S	1	Fine.
27	7	0	61,0	70,0	29,92	0,540	S by W	1	Fine.
	2	0	71,0	70,0	29,95		SW	2	Fine.
28	7	0	58,0	65,0	29,88	0,192	WSW	1	Fine.
	2	0	69,5	68,0	29,92		SW	1	Fine.
29	7	10	62,0	65,0	29,83	0,045	WSW	2	Rainy.
	2	0	68,0	68,0	29,79		SW	2	Fair.
30	7	0	60,5	66,5	29,94	0,045	W	1	Fine.
	2	0	72,5	69,0	30,01		NW	1	Fair.
31	7	15	60,0	66,5	30,01		W by S	1	Fair.
	2	0	70,0	68,0	30.03		NW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str	
Aug. 1	7	0	56,5	64,5	30,12	0,068	N by W	1	Fair.
	2	0	69,0	67,5	30,20		NNE	1	Fine.
2	7	0	53,5	63,0	30,32		NNE	1	Fine.
	2	0	67,0	64,5	30,31		SE	1	Fine.
3	7	0	55 0	62,5	30,22		S by W	1	Fine.
	2	0	73,0	65,5	30,15		N	1	Fine.
4	7	0	59,0	65,0	29,96	0,030	E by N	2	Fair.
	2	0	68,5	68,0	30,02		NE	1	Fair.
5	7	0	56,5	64,0	29,88	1,002	NE	2	Fair.
	2	0	62,5	65,5	30,00		NW	1	Fair.
6	7	0	59,0	63,5	30,09	0,042	SW	1	Cloudy.
	2	0	73,0	68,0	30,095		WSW	2	Fine.
7	7	0	63,5	66,0	30,09		WSW	2	Fine.
	2	0	78,0	70,5	30,11		WSW	2	Fine.
8	7	0	61,5	67,0	30,11		N	1	Fair.
	2	0	77,0	73,0	30,04		E by S	1	Fair.
9	7	10	67,5	70,5	29,81		S	1	Fair.
	2	0	77,5	74,0	29,80		WSW	1	Fine.
10	7	0	57,0	68,5	29,88		WSW	1	Fine.
	2	0	69,5	68,5	29,91		W	1	Fair.
11	7	0	54,0	63,0	30,08	0,138	SW	1	Fine.
	2	0	69,0	66,0	30,17		WSW	1	Fine.
12	7	0	54,5	62,0	30,22		SW	1	Fine.
	2	0	69,0	66,5	30,23		NW	1	Fine.
13	7	0	59,0	62,0	30,20		SW	1	Fair.
	2	30	71,5	68,5	30,20		W	1	Fine.
14	7	0	59,0	69,5	30,02		SW	1	Fine.
	2	0	73,0	68,5	29,92		SW	1	Fine.
15	7	0	59,0	65,5	29,68	0,106	SSW	1	Fine.
	2	0	68,0	67,5	29,63		SSW	2	Fair.
16	7	0	59,0	64,5	29,81	0,065	SE	1	Rain.
	2	0	70,0	67,0	29,63		S	2	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 17	7	0	58,0	64,0	29,68	0,048	W by S	2	Fine.
	2	0	69,5	66,0	29,84		W	2	Fine.
18	7	0	56,5	63,5	29,97		WSW	1	Fine.
	2	0	57,0	64,0	29,99		SW	1	Fine.
19	7	0	55,5	62,5	30,19		N by E	1	Fair.
	2	0	71,5	66,5	30,21		SE	1	Fine.
20	7	0	60,0	63,5	30,14		E by N	1	Fine.
	2	0	71,5	69,0	30,10		ESE	2	Fine.
21	7	0	62,5	66,0	30,04		SE	1	Fine.
	2	0	77,0	68,5	30,03		SE	1	Fine.
22	7	0	61,0	66,5	29,98		ENE	1	Cloudy.
	2	0	69,5	68,5	30,00		NNE	1	Fair.
23	7	0	59,0	66,0	30,11		N	1	Cloudy.
	2	0	68,0	68,0	30,15		N	1	Fair.
24	7	0	56,0	64,5	30,17		NNE	1	Cloudy.
	2	0	65,0	65,0	30,16		N by E	1	Cloudy.
25	7	0	57,5	63,0	29,93		E by N	1	Fair.
	2	0	73,0	66,5	29,795		S	1	Fine.
26	7	0	59,0	65,0	29,55	0,544	W by S	1	Fair.
	2	0	70,5	67,5	29,62		SW	1	Fine.
27	7	0	55,5	64,0	29,56	0,291	W by S	1	Fine.
	2	0	63,0	66,0	29,64		NW	1	Thunder.
28	7	0	57,0	52,5	29,535	0,293	S by E	1	Cloudy.
	2	0	62,5	63,5	29,38		W by S	2	Fair.
29	7	0	52,0	60,5	29,64	0,453	SSW	1	Fine.
	2	30	57,0	61,5	29,72		S by W	1	Fair.
30	7	0	58,0	60,5	29,80	0,165	S by W	1	Rain
	2	0	67,0	63 0	29,62		SW	2	Cloudy.
31	7	0	63,5	64,0	29,82	0,095	S by W	2	Cloudy.
	2	0	67,0	66,0	29,86		SSW	2	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	64,5	66,5	30,01	0,022	SSW	1	Cloudy.
	2	0	67,5	69,5	30,02		SW	2	Fine.
2	7	0	64,0	67,5	29,825		S	1	Fair.
	2	0	68,0	69,0	29,90		W	1	Fair.
3	7	0	53,0	64,5	30,09	0,045	SSW	1	Fair.
	2	0	70,0	67,0	29,96		SSW	1	Fine.
4	7	0	61,5	64,0	29,85		SW	1	Fine.
	2	0	73,0	68,0	29,81		SW	1	Fine.
5	7	0	52,0	61,5	30,00	0,041	W by N	1	Fine.
	2	0	68,0	64,5	30,035		W	1	Fine.
6	7	0	60,5	62,5	29,83	0,068	S by W	1	Cloudy.
	2	0	70,0	67,0	29,90		WNW	1	Fair.
7	7	0	54,0	61,0	30,05	0,346	NW	1	Cloudy.
	2	0	62,0	62,5	30,11		NNW	1	Fair.
8	7	0	45,5	53,5	30,27		NNE	1	Fine.
	2	0	57,0	56,5	30,28		N by E	1	Fine.
9	7	0	42,5	49,5	30,15		NNE	1	Fine.
	2	0	60,0	55,5	30,08		N by E	1	Fine.
10	7	0	50,5	54,5	30,06		E by N	1	Fine.
	3	0	67,0	58,5	30,04		SSW	1	Fair.
11	7	0	52,5	57,5	30,01	0,116	WSW	1	Fine.
	2	0	66,0	61,0	30,01		W by S	1	Fine.
12	7	0	53,0	60,0	29,625	0,176	SSW	1	Fine.
	2	0	59,0	61,5	29,55		W by S	1	Cloudy.
13	7	0	54,0	59,0	29,405		SE	1	Cloudy.
	2	0	56,0	61,0	29,58		E by N	1	Rain.
14	7	0	51,5	57,0	29,77	0,097	NNW	1	Cloudy.
	2	0	61,0	64,0	29,80		NW	1	Cloudy.
15	7	0	55,0	58,5	29,80		N	1	Cloudy.
	2	0	61,5	60,5	29,87		N by E	2	Fine.
16	7	0	50,0	57,0	29,83		W	1	Fair.
	2	0	58,0	58,5	29,82		N	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	51,0	56,5	29,92	0,179	N	1	Fine.
	2	0	61,5	59,5	29,95		N by E	1	Fine.
18	7	0	53,0	57,5	30,03		NNE	1	Fair.
	2	0	61,5	60,0	30,08		NE	1	Fine.
19	7	0	43,5	54,5	29,98		NE	1	Fine.
	2	30	64,0	58,5	29,87		S	1	Fair.
20	7	0	48,5	56,0	29,68	0,309	WNW	1	Fine.
	2	0	59,5	58,5	29,75		W by N	1	Fine.
21	7	0	52,0	56,0	29,68		SSW	1	Fair.
	2	0	58,0	58,0	29,565		SSE	2	Rain.
22	7	0	48,0	55,5	29,48	0,468	SSE	1	Fine.
	2	0	59,5	58,0	29,29		S by W	2	Fair.
23	7	0	54,0	57,0	29,11	0,244	S by W	2	Fine.
	2	0	58,5	59,0	29,28		S	2	Fine.
24	7	0	55,0	58,0	29,40	0,235	SSE	1	Fine.
	2	0	64,0	60,0	29,42		SE	1	Fair. Thunder about 9 P.M.
25	7	30	53,5	58,5	29,50	0,695	W	1	Rain.
	2	0	59,5	60,5	29,53		W	1	Rain.
26	7	0	53,5	59,0	29,56	0,108	SSW	1	Cloudy.
	2	0	58,5	60,0	29,64		WSW	1	Fair.
27	7	0	47,0	57,0	29,87	0,076	NNE	1	Fog.
	2	0	59,5	59,0	29,90		E	1	Fine.
28	7	0	53,0	56,5	29,78	0,018	S by E	1	Rain.
	2	40	56,0	57,5	29,71		SE	1	Rain.
29	7	0	50,0	56,0	29,68	0,236	S	1	Fine.
	2	0	62,0	59,0	29,65		E	1	Fair.
30	7	0	53,5	58,0	29,53	0,264	S	1	Fine.
	2	0	56,0	59,5	29,53		S by W	1	Rain.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	49,0	57,5	29,47	0,347	S	1	Fine.
	2	0	58,5	61,0	29,44		SW	1	Rain.
2	7	20	53,5	57,5	29,40	0,354	SW	1	Fine.
	2	0	57,0	59,0	29,52		NW	1	Fair.
3	7	0	42,5	53,5	30,12	0,054	W	1	Fine.
	2	0	56,0	55,5	30,22		NW	1	Fine.
4	7	0	43,5	52,0	30,38		SW	1	Fine.
	2	0	59,0	55,0	30,41		S by W	1	Fine.
5	7	0	50,5	54,0	30,43		S	1	Fine.
	2	0	60,5	57,5	30,42		S by W	1	Fine.
6	7	0	52,0	54,0	30,27		SE	1	Rain.
	2	0	64,0	58,0	30,23		SW	1	Fine.
7	7	0	53,0	57,5	30,32		WSW	0	Fair.
	2	0	64,5	60,5	30,37		W	0	Fine.
8	7	0	44,5	56,0	30,57		S	0	Fine.
	2	0	57,5	57,5	30,57		NE	1	Fine.
9	7	40	43,0	53,0	30,49		NE	1	Fair.
	2	20	57,5	55,0	30,38		S	1	Fine.
10	7	0	47,0	54,0	30,22		SW	1	Fine.
	2	0	59,0	56,0	30,18		W	1	Fine.
11	7	0	42,5	52,5	30,27		N by E	0	Fine.
	2	0	57,5	55,0	30,27		WSW	1	Fine.
12	7	0	48,5	54,0	30,29		WSW	1	Fair.
	2	0	59,0	56,0	30,30		NW	2	Fine.
13	7	0	50,5	55,0	30,40		W	0	Fair.
	2	0	58,5	57,0	30,43		N by E	1	Fair.
14	7	0	52,0	54,5	30,46		NE	1	Fair.
	2	0	58,0	57,5	30,46		NE	1	Fine.
15	7	0	41,5	52,0	30,44		NE	1	Fine.
	2	0	55,0	54,0	30,40		E	1	Fine.
16	7	20	44,0	51,5	30,395		ENE	1	Fine.
	2	0	57,0	53,5	30,24		SE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	42,5	51,5	30,12		ENE	1	Fog.
	2	0	54,0	53,5	30,14		S by E	1	Fine.
18	7	0	47,0	52,5	30,26		S by W	1	Fog.
	2	0	58,5	55,0	30,23		SW	1	Fair.
19	7	0	52,0	54,5	30,27		WSW	1	Fair.
	2	0	59,0	56,5	30,27		SW	1	Fair.
20	7	0	48,5	55,0	30,31		SSW	1	Fine.
	2	0	60,5	59,0	30,28		S	1	Fine.
21	7	0	44,5	53,5	30,24		N	1	Fair.
	2	0	60,5	56,0	30,22		S by E	0	Fine.
22	7	0	50,0	54,5	30,15		S by W	0	Fair.
	2	20	60,5	57,5	30,05		SW	1	Fair.
23	8	0	52,0	56,0	29,95	0,089	NW	1	Rain.
	2	0	50,0	55,0	29,95		SE	1	Rain.
24	7	0	45,0	52,5	29,80	0,246	NW	1	Fair.
	2	20	54,5	53,0	29,80		S by W	1	Fine.
25	7	0	41,0	50,0	29,87		SW	1	Fine.
	2	0	51,5	51,5	29,92		NW	1	Fine.
26	7	0	36,0	47,5	30,10		SW	1	Fine.
	2	15	51,5	49,0	30,15		NW	1	Fine.
27	7	0	38,5	46,5	30,15		N	1	Fine.
	2	0	52,0	48,5	30,12		N by E	1	Fine.
28	7	0	38,5	45,5	30,11		NNE	1	Fine.
	2	0	49,0	48,5	30,08		NE	1	Fair.
29	7	0	39,0	45,5	29,87	0,076	NE	1	Cloudy.
	2	0	45,0	47,0	29,74		E by N	1	Fair.
30	8	0	44,5	45,5	29,54	0,010	E by N	1	Rain.
	2	0	49,5	47,5	29,53		E by N	1	Fair.
31	7	0	48,5	49,0	29,50	0,172	E	2	Cloudy.
	2	20	54,0	51,0	29,54		ESE	1	Rain.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 1	7	0	51,5	52,0	29,75	0,082	NE	0	Fog.
	2	0	58,5	54,0	29,86		SSE	1	Fair.
2	7	30	49,0	53,0	30,00	0,030	E by N	1	Fog.
	2	0	53,0	53,5	30,00		E by N	1	Fair.
3	7	30	52,5	54,0	29,97		SE	0	Fog.
	2	0	58,0	56,0	29,98		SSW	1	Fair.
4	7	30	47,5	53,5	30,01		E by N	1	Fog.
	2	20	54,0	54,5	29,96		ENE	1	Fair.
5	7	30	48,5	52,5	29,71		SE	1	Fair.
	2	0	50,0	52,5	29,56		E	1	Fair.
6	8	10	47,0	51,0	29,17	0,580	E by N	2	Rain.
	2	15	48,5	51,5	29,20		E	2	Rain.
7	7	30	41,5	47,5	29,51	0,313	E by N	1	Fair.
	2	0	44,0	48,0	29,57		NE	1	Fair.
8	7	30	40,0	45,0	29,455		NNE	1	Fair.
	2	0	44,0	46,0	29,44		N	2	Fair.
9	7	30	39,0	43,5	29,50		NNE	1	Fair.
	2	30	42,0	44,5	29,53		ENE	2	Rain.
10	7	30	33,0	42,0	29,945	0,034	N	1	Fair.
	2	0	41,5	43,0	30,125		N	1	Fine.
11	7	30	35,0	40,5	30,05		NE	1	Fair.
	2	0	40,0	41,5	29,92		ENE	1	Little ſnow.
12	7	30	32,5	39,0	29,935	0,063	SW	1	Fog.
	2	0	41,0	41,0	29,86		SW	1	Fair.
13	7	35	33,0	38,0	30,18		N	1	Fine.
	2	0	40,5	40,0	30,225		NNW	1	Fine.
14	7	30	34,0	37,5	30,18		SW	0	Rain.
	2	0	41,0	41,0	30,19		SW	1	Fair.
15	7	30	42,5	42,0	30,11		S by W	1	Fair.
	2	0	49,0	44,5	30,07		WSW	1	Rain.
16	7	40	47,5	46,5	30,06	0,010	SSW	1	Fine.
	2	0	56,0	49,0	30,07		W by S	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 17	7	3	48,0	50,0	30,07		SW	1	Fair.
	2	0	52,5	51,0	29,97		SW	1	Fine.
18	8	0	51,0	51,5	29,55	0,115	W by S	2	Fair.
	2	15	44,0	50,0	29,66		W by N	2	Fair.
19	7	40	34,5	43,5	29,81		NW	2	Fine.
	2	0	39,5	43,0	29,82		W by N	1	Fine.
20	8	30	33,0	40,0	29,96		N	1	Fine.
	2	0	39,0	41,0	30,00		N	1	Fine.
21	7	30	33,5	38,5	30,11	0,024	N	1	Fair.
	2	0	36,5	39,0	30,07		N	1	Fair.
22	7	30	33,0	37,5	30,00		W by S	1	Snow.
	2	0	34,5	37,5	29,74		SSW	1	Snow and Rain.
23	7	30	35,5	39,0	29,865	0,181	E	1	Fair.
	2	20	38,0	40,5	29,865		W by S	1	Fair.
24	7	30	40,0	41,0	29,465	0,081	N by W	2	Fair, much wind laſt night.
	2	15	35,0	41,0	29,38		N	2	Rain and Snow.
25	8	0	32,5	37,5	29,635	0,094	NNE	1	Snow, windy night.
	2	0	36,0	39,0	29,76		N	2	Fair.
26	8	0	32,5	37,5	29,94	0,020	N	1	Fair.
	2	0	35,5	38,5	29,98		N	1	Fine.
27	8	0	31,5	36,0	30,14		N	2	Fair.
	2	0	35,0	36,5	30,11		N	1	Fair.
28	8	0	32,5	34,5	29,74		S	1	Snow.
	2	0	41,0	36,5	29,48		S	1	Fair.
29	8	0	33,0	36,5	29,28		SW	1	Fair.
	2	0	39,0	37,5	29,24		SW	1	Fair.
30	8	0	32,5	36,0	29,31		SW	1	Fine.
	2	30	38,5	37,5	29,40		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	31,0	36,5	29,57		W by S	1	Fair.
	2	0	38,5	37,5	29,62		NW	1	Fine.
2	8	0	34,0	36,5	29,45		SE	1	Fair.
	2	20	40,0	39,5	29,25		SSE	1	Rain.
3	8	0	45,5	40,5	29,11	0,283	SE	1	Rain.
	2	0	48,0	43,0	29,15		SSE	1	Rain.
4	8	0	44,5	45,5	29,48	0,068	E	1	Fair.
	2	0	49,0	47,0	29,68		SE	1	Fair.
5	8	0	43,0	47,0	29,84	0,075	ENE	1	Fair.
	2	0	47,0	47,5	29,90		E	1	Fine.
6	8	0	39,0	43,5	30,13		E	1	Fog and Rain.
	2	0	35,5	41,5	30,18		NE	2	Fine.
7	8	0	31,0	36,5	30,39		NE	1	Fair.
	2	0	32,0	36,5	30,35		NE	1	Fine.
8	8	0	28,5	33,5	29,99		N	1	Fine.
	2	0	29,5	34,0	29,88		N	1	Fine.
9	8	0	25,0	32,5	29,61		NW	1	Fair.
	2	0	27,0	32,5	29,61		NW	1	Snow.
10	8	0	29,0	30,5	29,34		NE	1	Fair.
	2	0	34,0	33,5	29,32		E by N	1	Rain.
11	8	0	45,5	37,5	29,40	0,327	S	2	Rain.
	2	0	51,5	41,0	29,54		S	2	Fair.
12	8	0	50,5	44,5	29,71		SSW	1	Fair.
	2	0	53,5	46,5	29,71		SE	1	Fine.
13	8	0	44,0	47,5	30,08		S	1	Fine.
	2	0	49,5	48,5	30,10		SW	1	Fine.
14	8	0	47,5	48,5	30,02	0,089	W by S	0	Fog.
	2	0	50,0	49,0	29,98		SW	0	Rain.
15	8	0	42,0	48,0	29,935	0,927	SW	1	Fog.
	2	0	48,5	49,0	30,09		W by N	1	Fine.
16	8	0	37,5	45,5	30,22		NE	1	Fog.
	2	0	43,5	46,5	30,16		SE	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	40,5	44,5	30,08		E	0	Fog.
	2	30	44,0	46,5	30,09		ENE	1	Fog, ſmall rain.
18	8	0	42,5	44,5	30,22		W	0	Fog.
	2	30	47,0	45,5	30,26		SW	1	Fair.
19	8	0	45,0	46,0	30,31		NW	1	Fog.
	2	0	47,5	47,0	30,31		NW	1	Fair.
20	8	0	39,5	45,5	30,30		NE	1	Fog.
	2	0	40,0	45,0	30,30		NE	1	Fair.
21	8	0	37,0	42,5	30,21		NNE	0	Fog.
	2	0	38,5	42,5	30,19		NNE	0	Fog.
22	8	0	34,0	40,5	30,35		NNE	1	Fine.
	2	0	40,5	41,0	30,42		NNE	1	Fine.
23	8	0	37,0	40,5	30,65		N	0	Fog.
	2	0	40,0	41,0	30,70		N	1	Fair.
24	8	0	39,5	33,5	30.70		N	1	Fair.
	2	10	37,0	40,0	30,71		NNE	1	Fine.
25	8	15	27,0	36,0	30,675		W	1	Fog.
	2	0	30,5	36,0	30,65		W by N	1	Fog.
26	8	0	34,5	36,0	30,65	0,010	W	0	Fog.
	2	0	36,5	36,5	30,61		W	0	Fog.
27	8	0	35,5	37,0	30,58		W	0	Fog.
	2	0	36,5	37,5	30,56		W by N	1	Fair.
28	8	0	36,0	37,5	30,47		W	0	Fog.
	2	0	39,0	38,5	30,39		W	1	Fair.
29	8	0	35,0	38,5	30,49		W	1	Fine.
	2	0	40,0	39,5	30,545		W	1	Fine.
30	8	0	28,0	36,5	30,56		NW	1	Fog.
	2	0	34,0	36,5	30,49		NW	1	Fine.
31	8	0	27,0	33,5	30,21		W by N	1	Fine.
	2	0	36,0	35,0	30,04	0,027	W by N	1	Fair.

1774	Thermometer without.					Therm. within.			Barometer.		
	Greateſt height.	Leaſt height.	Mean height A. M.	Mean height P. M.	Mean whole day.	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.
January	50,5	24,0	30,0	37,7	35,3	50,5	27,0	37,4	30,175	28,79	29,57
February	52,0	24.5	37,6	43,5	40,5	50,0	33,0	42,4	30,46	29,16	29,806
March	60,0	33,5	39,6	50,5	45,0	61,5	38,0	47,4	30,33	29,14	29,82
April	67,0	36,5	44,9	54,8	49,8	60,0	45,0	51,3	30,24	29,33	29,786
May	69,0	45,0	49,7	59,9	54,8	61,5	51,0	55,9	30,175	29,34	29,871
June	77,5	52,0	59,1	68,4	63,7	71,0	59,0	64,6	30,34	29,47	29,90
July	83,5	55,5	59,7	70,1	64,9	73,5	50,0	65,9	30,36	29,61	30,00
Auguſt	78,0	73,0	58,2	69,2	63,7	73,0	52,5	65,6	30,32	29,38	29,954
September	73,0	42.5	52,6	62,1	57,3	69,5	49,5	59.7	30,28	29,11	29,795
October	64,5	36,0	46,0	56,3	51,1	61,0	45,5	53,6	30,57	29,40	30,13
November	58,5	41,0	39,2	43,5	41,3	56,0	34,5	43.7	30,225	29,17	29,807
December	53,5	25,0	37,3	40,8	39,7	49,0	30,5	40,8	30,71	29,11	30,09

inch.

The quantity of rain in the whole year was 26,328, or about 26$\frac{1}{3}$ inches.

FOR

FOR THE VARIATION OF THE MAGNETIC NEEDLE.

OBSERVATIONS.

		Morning.	Noon.	2 P. M.	Evening.
		° ′	° ′	° ′	° ′
Auguſt	21	21 27			
	22	21 20		21 37	21 15
	23	21 28	21 38	21 24	21 18
	24	20	33	33	18
	25	41	35	35	19
	26	22	32	32	20
	27	21	35	29	20
	28	20	32	10	20
	29	15	33	15	02
	30	34	34	15	14
	31	23		34	12
September	1	27	34	34	12
	2	22	35	32	19
	3	41	36	33	18
	4	21	16	34	29
	5	24			
		406	393	397	236
		21 25	21 33	21 28	21 17

The mean of all,	21 26
Error of inſtrument,	10
Correct variation,	21 16 W.

How the error of the inſtrument was found, will be ſhewn in the next publication.

THE END OF VOL. LXV. PART I.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, and Labours,

OF THE

INGENIOUS,

IN MANY

CONSIDERABLE PARTS OF THE WORLD.

VOL. LXV. For the Year 1775.

PART II.

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PHILOSOPHICAL TRANSACTIONS.

XVI. *An abridged State of the Weather at* London *in the Year* 1774, *collected from the Meteorological Journal of the* Royal Society. *By* S. Horſley, *LL.D. Sec. R. S.*

ALTHOUGH the practice of keeping meteorological journals is, of late years, become very general, no information of any importance hath yet been derived from it. The reaſon of which perhaps may be, that after great pains and attention beſtowed in regiſtering particulars, as they occur, with a ſcrupulous minuteneſs, obſervers have not taken the trouble to form, at proper intervals of time, compendious abſtracts of their records, exhibiting the general reſult of their obſervations in each diſtinct branch of meteorology. The following tables are given as an example of the method that may be taken in future to remedy this neglect. With the general ſtate of the barometer and thermometer, already given at the end of the meteorological journal, they form a hiſtory of the weather at London during the laſt year. If the example were to be followed, in different parts of the kingdom, we might in time be furniſhed with an experimental hiſtory of the weather of our iſland.

TABLE I.

An abridged View of the WINDS at LONDON, in the Year 1774,

Compiled from the Meteorological Journal of the Royal Society.

	N	S	E	W	NW	SE	NE	SW		Rain.	
January	1½	0	2	1½	4	2½	4	13	31	2,958	Five half-days omitted in the Journal.
February	1	1½	1	3	2	0	3½	16	28	2,360	
March	2	1½	1	1	½	3	14	7½	31	1,780	Half of a day miſſed in the Journal.
April	2½	3	2	4½	2½	2	5	8½	30	1,242	
May	3	½	3½	0	6	3	10½	4	31	1,413	An half-day miſſed in the Journal.
June	½	3½	2½	2½	4	2	1½	13½	30	2,273	
July	½	2	0	1	6½	2	1	18	31	2,438	
Auguſt	2	1½	0	1½	2	4	6	14	31	3,340	
Septemb.	1½	2½	1	2½	4	3½	6	9	30	3,917	
October	1	2½	1	2	3½	3	8	10	31	1,215	
Novemb.	7	1	1½	0	2½	1½	7	9½	30	1,586	
December	2½	1½	2	4½	6	3½	7½	3½	31	1,806	
	25	21	17½	24	43½	30	74	126½		26,328	

This table ſhews the number of days that each wind blew in each month, dividing the compaſs only into eight points, and reckoning all the winds between N. and W., N.W.; all between S. and E., S.E; all between N. and E., N.E.; and all between S. and W., S.W. The number of days that each blew in all the months being collected into one ſum at bottom, ſhews the number of days each wind blew in the whole year. The quantity of rain that fell in each month is added, that the connection between wet and dry, and the ſeveral winds may the more readily appear. It appears that the winds from the S.W. prevailed more than any other in the year 1774; and next to the S.W. the N.E. But the S.W. was more frequent than the N.E. in the proportion of 126 to 74. Of the winds from the four cardinal points the North was the moſt frequent, and the Eaſt the moſt rare. In the three ſummer months, June, July, and Auguſt, there fell more rain than in the three of any other ſeaſon. Of the 26,328 inches of rain which fell in the whole year, 13,842 fell in the winter half-year, conſiſting of the ſix months of September, October, November, December, January, and February, and 12,486 in the ſummer half-year, conſiſting of the ſix months of March, April, May, June, July, and Auguſt. So that the winter's rain exceeded the ſummer's by $\overset{\text{inch}}{1{,}356}$; that is, by little more than $\frac{1}{10}$th part of half the rain of the whole year. September gave the greateſt quantity of rain, and October the leaſt of any ſingle month in the whole year.

In collecting the rain of the ſeveral months, my rule, with reſpect to what hath ſometimes fallen in the night between the laſt day of one month and the firſt of the next following, hath been this. When it appears by the journal, that it was fair on the laſt day of the month, at the time of the afternoon obſervations, I have given the whole of the enſuing night's rain to the new month; but if it rained on the laſt day of the month, at the time of the afternoon obſervation, I have divided the night's rain equally between the new month and the old one. For inſtance, it appears by the journal that 0,043 fell in the night between the laſt day of February and the firſt of March. The whole of this I have placed to the account of March; becauſe it was fair at the time of the afternoon obſervation on the laſt day of February. Again, in the night between the laſt day of September and the firſt of October, there fell 0,347. Half of this I give to September's rain and half to October's; becauſe it rained the laſt day of September at the time of the afternoon obſervation.

TABLE II.

Sub-diviſion of the S.W.	WSW	SW	SSW	
January	2	9½	1½	13
February	4	7½	4½	16
March	1½	4½	1½	7½
April	3	3	2½	8½
May	½	2	1½	4
June	1	9	3½	13½
July	5	9	4	18
Auguſt	5½	4½	4	14
September	2	1½	5½	9
October	2	5	3	10
November	2½	5	2	9½
December	1	2	½	3½
	30.	62½	34.	126½

TABLE III.

Sub-diviſion of the N.E.	ENE	NE	NNE	
January	2½	½	1	4
February	0	2½	1	3½
March	2½	9	2½	14
April	½	3½	1	5
May	2	8½	0	10½
June	0	½	1	1½
July	0	½	½	1
Auguſt	2	1	3	6
September	1	1	4	6
October	2½	3½	2	8
November	4	1½	1½	7
December	1½	3½	2½	7½
	18½	35½	20	74

In theſe two tables the winds between the W. and the S.W. are all ſet down to the W.S.W.; and thoſe between the S. and the S.W. are all reckoned S.S.W. In like manner, the winds between the E. and N.E. are all reckoned E.N.E.; and thoſe between the N. and N.E. are all reckoned N.N.E. It appears that of the winds between the S. and W. thoſe from the point of S.W. were far more frequent than thoſe from either ſide of it. And the winds from the point of N. E. more frequent than thoſe on either ſide of it, nearly in the ſame proportion.

TABLE IV.

Sub-diviſion of the S.E.

	ES	SE	SSE	
January	1½	1	0	2½
February	0	0	0	0
March	½	1½	1	3
April	0	1	1	2
May.	0	0	3	3
June.	0	1½	½	2
July	½	0	1½	2
Auguſt	1	2½	½	4
September	0	1½	2	3½
October	½	1½	1	3
November	0	1	½	1½
December	0	2½	1	3½
	4	14	12	30

TABLE V.

Sub-diviſion of the N.W.

	WNW	NW	NNW	
January	½	2½	1	4
February	1	1	0	2
March	0	½	0	½
April	½	1½	½	2½
May	½	3½	2	6
June	1½	1½	1	4
July	2	4½	0	6½
Auguſt	0	1½	½	2
September	2	1	1	4
October	0	3½	0	3½
November	1	½	1	2½
December	2½	3½	0	6
	11½	25	7	43½

By theſe two tables it appears, that of all the winds between the N. and W. thoſe from the point of N.W. were far more frequent than thoſe from either ſide of it. Of the winds between the S. and E. thoſe from the point of S.E. were more frequent than thoſe to the E. of that point, and rather more frequent than thoſe to the S. of it; but the difference in the latter caſe was very inconſiderable. Of the winds from all quarters, thoſe from the E.S.E and N.N.W. were the moſt rare, eſpecially the former. The numbers in the laſt columns of each of the four laſt tables are the ſums of the preceding columns ranging in the ſame horizontal lines. They ought to correſpond with the numbers in columns S.W. N.E. S.E. N.W. of TABLE I. reſpectively, and ſerve as a check upon the work in making the tables.

The general ſtate of the winds collected from the five preceding tables, according to their different degrees of prevalence, is as follows:

ESE	NNW	WNW	SSE	SE	E	ENE	NNE	S	W	NW	N	WSW	SSW	NE	SW	
4	7	11½	12	14	17½	18½	20	21	24	25	25	30	34	35½	62½	361½

Days miſſed in the Journal, - - - - - - 3½

365

TABLE VI.

Shewing the number of fair and froſty days in each half month and in the whole year.

	Fair		Fair days in whole month.	Froſty days		Froſty days in whole month.
	1ſt half.	Latter half.		1ſt half.	Latter half.	
January	9	6	15	10	7	17
February	7	3	10	6		6
March	7	13	20	4		4
April	11	8	19			
May	8	6	14			
June	10	6	16			
July	6	8	14			
Auguſt	9	8	17			
September	7	2	9			
October	12	10	22			
Novemb.	5	6	11		1	1
December	6	13	19	5	3	8
Total fair days,			186	Total froſt,		36

There were but 10 days in the whole year that gave any ſnow; *viz.* 3 in January, 1 in February, 5 in November, and 1 in December. The firſt ſnow on the 9th of January in the afternoon, after a rainy morning, ſet in with a N.N.E. wind, and was ſucceeded by a ſharp froſt for three days and a half, with the wind E.N.E. The ſecond, which happened in the night between the ſeventeenth and eighteenth, came likewiſe after rain, and was ſucceeded by a froſt of four days and a half, wind ſhifting between N.W. and S.E. The laſt ſnow in January, on the 24th, fell with a S.W. wind, which ſet in the day before. It was followed by a moderate froſt of one day, though the wind continued in the S.W. The ſnow on the 1ſt of February came with a S.W. during a ſharp froſt. The wind was in the N.E. before the ſnow, and returned to the ſame point the next morning; the froſt ſharper than before the ſnow. The ſnows in the latter part of November were generally accompanied with rain, and did not bring actual froſt. The ſnow on the 9th of December came after two days froſt, which it ſeems to have put an end to. For though it froze in the evening after the ſnow, the froſt was much leſs ſevere than the preceding night, and a thaw came with rain, wind N.E., the next day.

There were only two thunder ſtorms this year, *videlicet*,

Auguſt 27. 2 P.M. Barometer 29,64 inches, Thermometer 63°, Wind N.W.

September 24. 9 P.M. Barometer 29,42 inches, Thermometer at 2 P.M. 64°.

TABLE VII.

For Trial of the Influence of the WINDS upon the BAROMETER.

	SW	WSW	SSW	NE	ENE	NNE	SE	ESE	SSE	NW	WNW	NNW	N	S	E	W
Jan.	29,37	29,61	29,60	29,34	29,64	29,58	29,57	29,72		29,66	29,895	29,675	29,72		29,80	29,67
Feb.	29,63	29,81	29,70	30,30		30,41				29,73	29,615		29,69	29,86	29,85	29,90
Mar.	29,39	29,79	29,89	29,945	29,87	30,20	29,52	29,88	29,52	29,615			30,14	29,87	29,90	29,75
April	29,77	30,07	29,61	29,95	30,21	29,80	30,00		29,69	29,80	29,58	29,90	29,82	29,475	29,53	29,80
May	29,91	29,46	29,45	29,96	29,86				29,71	29,81	29,97	30,04	29,98	29,34	29,925	
June	29,91	29,92	29,80	30,21		29,75	29,92		29,95	30,00	29,69	29,87	29,60	29,83	29,84	30,09
July	29,97	29,97	29,98	29,92		30,33		29,78	29,97	30,07	30,085		30,26	30,25		29,94
Aug.	29,97	29,85	29,80	29,95	30,00	30,17	30,05	30,07	29,535	29,960		30,12	30,13	29,74		29,98
Sept.	29,89	29,80	29,70	29,97	29,82	29,94	29,51		29,56	29,925	29,82	29,94	29,85	29,64	29,775	29,76
Oct.	29,98	30,28	30,225	30,32	29,865	30,23	30,15	29,54	30,18	29,98			30,19	30,22	29,95	30,27
Nov.	29,74	29,91	29,92	29,79	29,73	29,53	29,84		29,86	29,81	29,74	29,90	29,93	29,61	29,54	
Dec.	30,07	29,795	29,71	30,15	29,82	30,38	29,62		29,20	30,21	30,31		30,38	29,64	29,90	30,51
Means	29,76	29,92	29,80	30,02	29,82	30,01	29,80	29,81	29,70	29,94	29,93	29,92	29,99	29,80	29,80	30,02

It is an old obſervation, that a N.E. wind in this country generally makes the barometer riſe. This naturally leads to an enquiry, whether there be any general connection of the riſe and fall of the barometer with the ſetting of the wind. Upon comparing the general account of the barometer for the year 1774, as ſtated at the end of the meteorological journal, with the journal at large, I found, that in ſeven months out of the twelve the greateſt height of the barometer was accompanied with a North-eaſterly wind; and in eight months out of the twelve, the leaſt height of the barometer was accompanied with a S.W. This incited me to take the trouble of making out the preceding table, which ſhews the mean height of the barometer which accompanied each wind in every month, and for the whole year. And it appears, that though the barometer may be almoſt at any height with any wind, yet the mean height was greater, in the courſe of the laſt year, with the winds which ſet from that ſemicircle of the compaſs, which is intercepted between the points of W.S.W. incluſive and E.N.E. excluſive, going round by the W. and N. than with the winds which ſet from the oppoſite ſemi-circle intercepted between E.N.E. incluſive and W.S.W. excluſive, going round by E. and S. In the former ſemi-circle the W. and N.E. give the greateſt mean height, and in the latter the S.S.E. and S.W. give the leaſt*.

---

* It is to be noted, that the means of the whole year, ſtated in the lowermoſt horizontal row, are not found by collecting the means of all the months into one ſum, and dividing by the number of months (for this method would always be fallacious, except each wind had blown for the ſame number of days in all the different months); but by adding together the heights attending each wind day by day, and dividing the ſum by the number of days each wind blew in the whole year.

TABLE VIII.

For Trial of the MOON's Influence.

	Laſt Qr.		New.		Firſt Qr.		Full.		
	D.	H.	D.	H.	D.	H.	D.	H.	
Jan.	5	6	11	21	19	3	27	7	〰+6+— ⊙—7 〰+***9** ⊙—10 +〰14 ⊙—18 〰+⊙**23** —25 〰26 ⊙30
Feb.	3	15	10	9	18	0	25	23	〰**4** +7 〰⊙**8** 〰**10** ⊙**13** 〰15 ⊙**17** 〰20
Mar.	4	22	11	22	19	20	27	11	⊙10
Apr.	3	5	10	12	18	15	25	22	〰**4** ⊙**8** 〰28 ⊙**29**
May	2	12	10	3	18	7	25	5	〰5 ⊙**7** 〰23
							Laſt Qr.		
							31	20	
	New.		Firſt Qr.		Full.		Laſt Qr.		
June	8	18	16	19	23	12	30	7	⊙**6** 〰**17** ⊙18 〰**20** ⊙22 〰**28** ⊙**30**
July	8	9	16	5	22	19	29	20	〰5 ⊙**15** 〰20 ⊙22 〰**27** ⊙**31**
Aug.	7	0	14	12	21	3	28	12	〰**4** ⊙6 〰⊙11 〰[illegible]**5** ⊙17 〰**26**
Sept.	5	14	12	17	19	13	27	7	⊙1 〰**5** ⊙7 〰11 ⊙**13** 〰⊙14 〰⊙**17** 〰⊙**20** 〰22
Oct.	5	3	12	0	19	2	27	3	⊙**3** 〰23 ⊙**24** 〰29
Nov.	3	15	10	7	17	18	25	23	⊙2 〰6 ⊙**7** 〰⊙**18** 〰*—21 ⊙**26**
Dec.	3	2	9	17	17	12	25	17	〰2 ⊙5 〰⊙11 〰**14** ⊙15

This table exhibits a compariſon of the actual changes of the weather from fair to foul, with the aſpects of the Moon; and needs no other explanation than an interpretation of the characters in the laſt column.

— froſt
+ thaw
⊙ fair
~ rainy
≈ ſtormy
* ſnow

Any one of theſe marks placed over a number ſignifies, that the weather indicated by that mark continued from the day of the month denoted by the number underneath to the day denoted by the next following number, bearing ſome other mark over it. Thus, in the month of July, rainy weather ſet in on the fifth, and laſted to the fifteenth; from the 15th to the 20th it was fine; when it changed again, and continued rainy till the 22d; then it was fine to the 27th, and rainy again till the 31ſt.

Such tables of compariſon, made yearly for a ſucceſſion of years, would in the end decide with certainty for or againſt the popular perſuaſion of the Moon's influence upon the changes of our weather; which hath ſome how or other gained credit even among the learned, without that ſtrict empiric examination, which a notion in itſelf ſo improbable, ſo deſtitute of all foundation in phyſical theory, ſo little ſupported by any plauſible analogy, ought to undergo. The vulgar doctrine about this influence is, that it is exerted at the ſyzygies and quadratures, and for three days before and after each of thoſe epochs. There are 24 days therefore in each ſynodic month, over which the Moon at this rate is ſuppoſed to preſide; and as the whole conſiſts but of 29 days 12¾ hours, only $5\frac{1}{2}$ days are exempt from her pretended dominion. Hence, though the changes of the weather ſhould happen to have no connection whatever with the Moon's aſpects, though the fact ſhould be, that they take place at all times of the Moon indifferently, and are diſtributed in an equal proportion through the whole ſynodic month; yet any one who ſhall predict, that a change ſhall happen on ſome one of the 24 days aſſigned, rather than on any one of the remaining $5\frac{1}{2}$, will always have the chances 24 to $5\frac{1}{2}$ in his favour. Merely becauſe more changes will fall with in the greater time, and, upon an average, as many more in proportion as the time is greater. It is evident therefore, that this is a matter in which men may eaſily deceive themſelves, eſpecially in ſo unſettled a climate as that of this iſland: and the advocates for lunar influence are not to imagine they have fact on their ſide, unleſs it ſhould appear, from ſuch tables as theſe carefully kept for a long courſe of years, that the changes happening on the days, which they hold to be ſubject to the Moon, are more than thoſe which happen on the exempted days, in a much greater proportion than that of 24 to $5\frac{1}{2}$.

The antiquity of the opinion may perhaps be alledged in its favour; and it may ſeem an anſwer to the objection taken from the inſtability of the weather of this part of the world, that it had its origin in more ſettled climates. We find it, it muſt be confeſſed, in the earlieſt Greek writers, who probably had it with the reſt of their phyſics from the Eaſt. And to this circumſtance, I am perſwaded, the opinion owes the credit it hath met with among men of learning. But whatever general aſſertions may be found in ſome writers, concerning celeſtial influences in general, and the Moon's in particular, as being of all the heavenly bodies the neareſt to the earth, the writers who treat of the ſigns of the weather practically, for the information of huſbandmen and mariners, derive their prognoſtics from circumſtances, which neither argue any real influence of the Moon as a cauſe, nor any belief of ſuch an influence; but are merely indications of the ſtate of the air at the time of obſervation: namely, the ſhape of the horns, the degree and colour of the light, and the number and quality of the luminous circles which ſometimes ſurround the Moon, and the circumſtances attending their diſappearance(a). It is true, that each of theſe prognoſtics is expreſsly confined, by the early writers, to a particular time of the Moon's age(b). But not, as I conceive, on account of any particular influence of the Moon in this or that aſpect; but merely becauſe the prognoſtics, that ſhe affords at one age, are ſuch in themſelves as ſhe cannot afford at another. For inſtance, the bluntneſs of the horns in the new Moon is a ſign of approaching rain, becauſe it indicates a turbid ſtate of the atmoſphere; for if the air were clear and dry, the horns ſhould appear ſharp and pointed, that being then their natural ſhape. But the bluntneſs of the horns is no ſign of change after the dichotomy; becauſe then the horns will appear blunt in all ſtates of the air, the elliptic arc on the deficient ſide of the Moon preſenting its concavity to the circular limb, and forming with it an obtuſe angle. Again, the degree of the Moon's light on the fourth day furniſhed a prognoſtic. It ought then to be ſtrong enough, if the air was clear, for terreſtrial objects to caſt a ſhadow(c). If their ſhadows were not diſcernible, it was a ſign that the air was impure, and bad weather was to be expected. But this prognoſtic did not take place *before* the fourth day, becauſe the light of the Moon was yet too weak for ſhadows to be formed in the pureſt ſtate of the air. It did not take place *after* the fourth day, becauſe the enlightened part was then ſo much encreaſed, that ſhadows would be formed in any ſtate of the air, if the Moon was not actually hidden by a cloud, or obſcured by ſenſible miſts. The prognoſtics furniſhed by the new Moon ſerved only till the dichotomy, and thoſe of the dichotomy till the full Moon, and ſo on; not becauſe a new and diſtinct influence was exerted in each new aſpect, but becauſe each new aſpect furniſhed a new ſet of ſigns, of a different kind. That this is a true repreſentation of the moſt ancient lunar prognoſtics, appears from hence; that others of a ſimilar kind were derived from the Sun and the fixed ſtars, particularly the *Præſepe and Aſelli* in Cancer, and the bright ſtar in the Altar.; and it is remarkable, that ARATUS ſays, the prognoſtics taken from the Sun are the moſt certain of all(d). The vulgar ſoon began to conſider thoſe things as cauſes, which had been propoſed to them only as ſigns. The manifeſt effect of the Moon upon the Ocean, while the mechanical cauſe of it was totally unknown, was interpreted as an argument of her influence over all terreſtrial things; and theſe notions were ſo conſiſtent with that viſionary philoſophy, which aſſigned diſtinct places to corruption, change, and paſſivity, on the one ſide, and the active governing powers of nature on the other, and made the orb of the Moon the boundary between the two, that they who ſhould have been its opponents, ranged themſelves on the ſide of popular prejudice. And the uncertain concluſions of an ill-conducted analogy, and a falſe metaphyſic, were mixed with the few ſimple precepts derived from obſervation, which probably made the whole of the ſcience of prognoſtication in its earlieſt and pureſt ſtate. Hence both THEOPHRASTUS and ARATUS teach us to remark the poſition of the Moon's horns, and take conjectures of approaching fair weather or tempeſt, according as they appear, at different times of the Moon's age, erect, reclined, or prone: not knowing

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(a) See the Διοσημεια of Aratus and the Scholia of Theon.

(b) Σήματα δ᾽ ἔτ᾽ ἄρ᾽ πᾶσιν ἐπ᾽ ἤμασι πάντα τέτυκται.
Ἀλλ᾽ ὅσα μὲν τριτάτῃ τετραταίῃ τε πέληται,
Μέσφα διχαιομένης· διχάδος γέ μεν ἄχρις ἐπ᾽ αὐτὴν
Σημαίνει διχόμηνον· αὐτὰρ πάλιν ἐκ διχομήνης
Ἐς διχάδα φθιμένην· ἔχεται δέ οἱ αὐτίκα τετρὰς
Μηνὸς ἀποιχομένης. Αρατ. Διοσημεια.

(c) ——ὅτε πρώτη ἀποσκίδναται αὐτόθεν αὐγὴ
Ὅσσον ἐπισκιάειν ἐπὶ τέτρατον ἦμαρ ἰοῦσα. Αρατ. Διοσημεια.

Τεταρταία γενομένη ἡ σελήνη ἄρχεται δύνασθαι σκιάζειν ἐν τῷ φωτὶ αὐτῆς· τριταία γὰρ οὐ δύναται διὰ τὴν περικειμένην τοῦ φωτὸς ἀδράνειαν. Theon in locum.

(d) Ηελίῳ ϗ μᾶλλον ἐοικότα σήματα κεῖται. Διοσημεια.

that the poſition of the line joining the Moon's cuſps, with reſpect to the horizon, depends merely upon the mutual approach, or receſs, of the pole of a great circle drawn through the centers of the Sun and Moon, and the pole of the horizon, in the courſe of the diurnal revolution. And ſo great a man as VARRO, as he is quoted by PLINY, was not aſhamed to give this childiſh rule, for predicting the weather, for a whole month to come, from appearances at the new Moon. "If the upper horn "be obſcure, the decline of the Moon will bring rain. "If the lower horn, the rain will happen before the full. "At the time of the full Moon, if the blackneſs be in "the middle(e)." After this one cannot be ſurprized, that the poet VIRGIL ſhould make the prognoſtics of the fourth day deciſive for the whole lunation:

Sin ortu quarto, namque is certiſſimus auctor,
Pura neque obtuſis per cœlum cornibus ibit,
Totus et ille dies, et qui naſcentur ab illo,
Exactum ad menſem pluviâ ventiſque carebunt.

Georgic. lib. I. lin. 143.

But in this he contradicts ARATUS, whoſe authority in general he follows implicitly. With ARATUS, the ſigns of the new Moon extend only to the firſt quarter.

The ancients aſcribed an influence to the conſtellations and fixed ſtars as well as to the Sun and Moon; and there ſeems to have been much the ſame foundation for one as the other. In the *parapegmata* or calendars, introduced in Greece, as we learn from THEON (f), by the aſtronomer METON, and renewed either annually or, as I rather conjecture, at the expiration of every 19-year period, the heliacal riſings and ſettings of different ſtars were marked as bringing in different ſorts of weather. The truth is, the earlieſt aſtronomers imagined, that the weather was governed by the Sun; and that its varieties were every where owing to the different degrees of the Sun's heat in the different ſeaſons. They had therefore taken great pains to collect, by a long ſeries of obſervations, the weather that uſually prevailed in this or that particular place during the Sun's paſſage through every degree of every ſign. Upon theſe obſervations, not upon any whimſical theory of celeſtial influences, the predictions in the calendars were founded. It ſeemed reaſonable to announce, as the weather of each part of the year, what had been found to be then moſt frequent. And while the civil reckonings of time were ſo different among the different Greek ſtates, and ſo rudely digeſted in all, the heliacal riſings and ſettings of the ſtars were the only certain and obvious marks, the compilers of thoſe popular directories could hit upon, of the Sun's return to the different parts of the zodiac (g). Hence they propoſed them to the people as ſignals of the weather to be expected. The form of the year being now the ſame in all parts of Europe, and pretty accurately adjuſted to the motions of the heavenly bodies, and the heliacal riſings and ſettings of the ſtars, from the different manner of life of our country people, not falling ſo much under popular obſervation with us, as they did among the Greeks, they are not marked as prognoſtics in our modern almanacks: and this I take to be the reaſon, that though the Moon hath maintained her reputation amongſt us, the influence of the fixed ſtars is ſunk, as it well deſerves, in utter oblivion. Upon the whole I do not deny, that the obſervant huſbandman will find a variety of uſeful prognoſtics in the appearances of the Moon, and the heavenly bodies in general; but they will be prognoſtics of no other kind, and for no other reaſon (though perhaps leſs fallible) than the ſputtering of the oil in the induſtrious maiden's lamp, or the excreſcences which gather round the wick(b). They will be ſymptoms deſtitute of all efficient powers. They will ſhew the preſent ſtate of the air, as that on which they depend, not as that which they govern, and may furniſh probable conjectures for two or three days to come. To what I have already advanced in ſupport of this opinion, I ſhall only add the laſt lines of the *Διοσημεια* of ARATUS. They ſpeak the ſentiments of the earlieſt ages moſt deciſively, as they ſhew how little the doctrine of the influence of lunar aſpect had gained ground, even in his days, among practical writers. That elegant verſifier, there is little room to doubt, delivers the practical maxims of his time, juſt as he received them. He was too little of a poet to diſguiſe the truth with ornamental fiction, and too little of a philoſopher to adulterate it with hypotheſis.

(e) Apud Varronem ita eſt.———— Naſcens Luna ſi cornu ſuperiore obatro ſurget, pluvias decreſcens dabit: ſi inferiore, ante plenilunium: ſi in mediâ nigritia illa fuerit, imbrem in pleno. PLIN. Nat. Hiſt. lib. XVIII. cap. 35.

(f) Scholia in Aratum.

(g) Geminus. Εἰσαγωγὴ εἰς τὰ φαινόμενα. c. 14.

(b) Ne nocturna quidem carpentes penſa puellæ
Neſcivere hiemem: teſtâ cum ardente viderent
Scintillare oleum, et putris concreſcere fungos.
Georgic. lib. I. lin. 390.

Ἠ λύχνοιο μύκητες ἀγείρωνται περὶ μύξαν,
Νύκτα κατὰ σκοτίην, μηδ᾽ ἢν ὑπὸ χείματος ὥρῃ
Λύχνου ἄλλοτε μέν τε φάος κατὰ κόσμον ὀρώρῃ,
Ἄλλοτε δ᾽ ἀΐσσωσιν ἀπὸ φλόγες, ἠΰτε κοῦφαι
Πομφόλυγες &c. Αρατ. Διοσημ.

Τῶν μηδὲν κατόκνησο, καλὸν δ' ἐπὶ σήματι σῆμα
Σκέπτεσθαι, μᾶλλον δὲ δυοῖν εἰς ταυτὸν ἰόντοιν
Ἐλπωρὴ τελέθοι· τριτάτῳ δέ κε θαρσησείας.
Αἰεὶ δ' ἂν περιόντος ἀριθμοίης ἐνιαυτοῦ
Σήματα, συμβάλλων εἴπου καὶ ἐπ' ἀστέρι τοίη
Ἠως ἀντέλλοντι κατέρχεται, ἢ κατιόντι,
Ὁπποῖον καὶ σῆμα λέγοι· μάλα δ' ἄρκιον εἴη
Φράζεσθαι φθίνοντος, ἐφισταμένοιό τε μηνὸς
Τετράδας ἀμφοτέρας· αἳ γάρ τ' ἄμυδις συνιόντων
Μηνῶν πείρατ' ἔχουσιν, ὅτε σφαλερώτατος αἰθὴρ
Ὀκτὼ νυξὶ πέλει, χήτεϊ χαροποῖο σελήνης.
Τῶν ἄμυδις πάντων ἐσκεμμένος εἰς ἐνιαυτὸν,
Οὐδέποτε σχεδίως κεν ἐπ' αἰθέρι τεκμήραιο.

Which I render thus: “ Neglect none of theſe prog-
“ noſtics [none, he means, of the great variety he hath
“ enumerated, taken from the heavens, from animals,
“ plants, terreſtrial objects, &c.], it is a good thing to com-
“ bine the obſervation of one prognoſtic with another. If
“ two agree, there is the greater likelihood of the event,
“ and a third makes it certain. Whatever you do, regiſter
“ [ἀριθμοίης] the prognoſtics of the current year, care-
“ fully noting what the prognoſtic ſays [ὁπποῖον καὶ σῆμα
“ λέγοι; that is, what the event ſhew it to be a ſign of],
“ if ſuch a ſort of morning (i) comes on with the riſe or
“ ſetting of any particular ſtar. And it will be of the

(i) *Such a ſort of morning.* That is, a morning marked with ſuch or ſuch appearances. So I underſtand τοίη ἠώς. The ſpirit of the precept ſeems to be, that the heliacal riſings of the ſtars are to be attended to, in conjunction with the particular appearances attending the dawn or ſun-riſe. The heliacal riſings ſhew the ſeaſon, or general conſtitution of the time of the year; the particular appearances of the morning indicate the minute circumſtances of the weather for two or three days to come. Thus the heliacal riſing of Arcturus was a ſign, in all the ancient parapegms, that the ſtormy ſeaſon was at hand, and bad weather of various ſorts, rain, thunder, high wind, was to be expected; but what the particular weather would be for a day or two to come, whether it would be only windy, or wet, with thunder or without, from what quarter the bad weather would come, all this would be pre-ſignified by the particular appearances of the morning. Perhaps the ſame appearance may be ſubject to ſome variety of interpretation at different ſeaſons of the year, and in different places. In this, experience and obſervation will be the only ſure guides. And for this reaſon ARATUS adviſes his ſcholar, not only to attend to the general rules laid down for him, but to keep a journal for himſelf, and make his own concluſions.

" higheſt importance to attend particularly to the two
" quaternions of the expiring and the incipient month(k)
" [that is to the four laſt days of the month going out,
" and the four firſt of that which is ſetting in], for they
" compriſe the extremities of the two months, where
" they meet: and the weather [or the ſtate of the air]
" is then particularly uncertain [difficult to gueſs at] for
" eight nights, for want of the ſilver-coloured Moon.
" If you attend to all theſe put together, all through the
" year, you will never form a random gueſs about the
" weather." The uncertainty of the weather for theſe eight nights cannot be an uncertainty of the effect depending upon the moon's aſpect; but it is an uncertainty of fore-knowledge, the poet ſpeaks of, for want of the Moon as an index. For though the word σφαλερώτατος by itſelf would be ambiguous, as it might be taken either in the ſenſe of δυσόχαστος or εὐμετάβλητος, the words χήτει χαροποῖο σελήνης are deciſive for the firſt interpretation. The moon exiſts during theſe eight months as at other times. There is no want of her therefore as a phyſical agent: the only want there can be, is the want of her

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(k) *And it will be of the higheſt importance to attend to,* &c. μάλα δ' ἄρκιον εἴη φράζεσθαι. I have ſometimes thought theſe words might be rendered thus: "This will be of great importance [that is, this joint obſervation of the general "indications of ſeaſon and of particular prognoſtics will be of great importance] "in order to *form a conjecture* about the two quaternions, &c." This interpretation would make the moſt connected meaning for the whole paſſage; but I do not recollect, nor can I find upon the ſtricteſt ſearch, any inſtance, wherein the verb φράζεσθαι is uſed in the ſenſe of *conjecturing*, or *forming a judgement or opinion about.*

appearance. It would be unpardonable not to mention, that ſo great an authority as that of THEOPHRASTUS is againſt the ſide of the queſtion to which I incline. The doctrine of the influence of lunar aſpect is expreſsly aſſerted in his Treatiſe on the Signs of Rain and Wind. He ſays, that the new Moon is generally a time of bad weather, becauſe the light of the Moon is wanting(l); and that the changes of weather generally fall upon the ſyzygies or quadratures. But this ſeems to have been merely an opinion founded upon an imaginary analogy between the epochs of ſyzygy and quadrature in the months, and the equinoxial and tropical epochs in the year. For the Moon, he ſays, is, as it were, the Sun of the night. THEOPHRASTUS, though a diligent obſerver of nature, was deep in the theory of that ſchool, of which he was himſelf one of the brighteſt ornaments: and his teſtimony, with reſpect to the matter of fact, hath not, like ARATUS's, a credibility founded on the mediocrity of his genius.

In the table, p. 177. the changes which fell on the ſyzygies and quadratures, or on any one of PLINY's critical days of the Moon's age (which are the 3d, 7th, 11th, 15th, 19th, 23d, 27th,), are diſtinguiſhed from the reſt by a larger character(m). And out of 69 changes regiſter-ed in this table 32 claim that diſtinction. Which is rather a larger proportion of the whole number, than is due to the time made up of all the days of ſyzygie and quadrature, in the whole year, together with PLINY's critical days, thrown into one ſum. For ſince there were 365 days in the year, and the days of ſyzygie and quadrature, with PLINY's critical days, amount to 113, out of 69 changes in the whole year 22 are as many as belong to theſe particular days, upon a proportional diſtribution. But in the preceding table, there are many alterations marked as changes, when it appears, that the weather returned to what it had been before the time of change, within the ſpace of 24 hours after it. Now if we reject all theſe on both ſides of the queſtion (which I think is the fair way of reckoning, for ſudden alterations, of ſo ſhort a duration, are rather to be called irregularities than changes of weather), we ſhall find but 46 changes in all, from one ſettled ſtate to another, of which only 20 fell on the days of ſyzygies, quadrature, or PLINY's days, which is ſtill more than the juſt proportion.

(l) Διὸ ϗ αἱ σύνοδοι τῶν μηνῶν χειμέριοί εἰσιν· ὅτι ἀπολείπει τὸ φῶς τῆς σελήνης, &c. THEOPHRAST. de ſignis Pluv. p. 417. Edit. Heinſ.

(m) Sunt et ipſius Lunæ octo articuli quoties in angulos ſolis incidit, pleriſque inter eos tantum obſervantibus præſagia ejus, hoc eſt tertia, ſeptima, undecima, decima quinta, decima nona, vigeſima tertia, vigeſima ſeptima, et interlunium. PLIN. Nat. Hiſt. lib. XVIII. c. 35.

But again. PLINY's eight critical days were probably intended for the four days of ſyzygie and quadrature and the four of octagonal aſpect(n). For if the time of the conjunction be rightly aſſumed, the mean quadratures, and the mean oppoſition, and the mean octagonal aſpect, will always fall either on one of PLINY's days, or on the day next to it. The deviation, I ſuſpect, was intentional, and for the ſake of the odd numbers. Thus the 4th, 8th, and 12th days of the Moon ſhould have been critical, inſtead of the 3d, 7th, and 11th, if the mean motions of the Moon had been the ſingle thing attended to. But PLINY, or whoever was the firſt author of the rule he gives us, choſe the latter as containing, beſides much of the lunar influence, all the magic virtue of imparity, of which the others, taking their numerical denomination from even numbers, are totally deſtitute. Among the numerous believers in the Moon of our days, few, I ſuppoſe, retain any confidence in the phyſical powers of the odd numbers. They may imagine therefore, that the apparent inconſiſtence of PLINY's rule with the truth of things, may be owing to his ſuperſtition about the odd numbers, which led him wilfully to deviate from the mean epochs, little apprized (for the Romans never were aſtronomers) how much they ſometimes differ from the true ones, on account of the great and various inequalities of the Moon's motions, and how very widely his arbitrary arrangement would in conſequence often differ from the times it was intended nearly to repreſent. Inſtead of PLINY's critical days, I ſhall now, therefore, examine the days for which, I imagine, they were ſubſtituted; thoſe I mean of true ſyzygie, true quadrature, and true octagonal aſpect. The following table diſtinguiſhes the changes of weather which fell on theſe days. There were only 22 ſuch out of all the 69; which is ſcarce four more than their even proportion. And rejecting, as before, on both ſides, the alterations of weather which were reverſed within the ſpace of 24 hours, there remain out of 46 changes in all only 10 upon the days of lunar influence, which are two leſs than belong to them upon the even chance; for the days of ſyzygie, quadrature, and octagonal aſpect, in the whole year are 98; and 365 : 98 = 46 : 12$\frac{1}{2}$ very nearly. It is remarkable, that of theſe ten changes two only coincide with a new Moon; namely, thoſe of the 10th of February and 5th of September, and none at all with a full Moon. There were indeed two changes in the year upon the day of the full Moon; *videlicet*, thoſe of the 20th of September and 18th of November; but both were reverſed within the ſpace of 24 hours.

(n) The words, *Quoties in angulos ſolis incidit*, imply this.

TABLE IX.

January	**6** 7 **9** 10 14 18 **23** 25 26 30	10	3
February	**4** **7** 8 **10** 13 15 17 20	8	3
March	10	1	0
April	4 8 28 **29**	4	1
May	5 7 23	3	0
June	6 **17** 18 **20** 22 28 **30**	7	3
July	5 15 **20** 22 27 31	6	1
Auguſt	4 6 **11** **15** 17 26	6	2
September	**1** **5** 7 11 **13** 14 17 **20** 22	9	4
October	3 **23** 24 29	4	1
November	2 6 **7** **18** 21 **26**	6	3
December	2 5 11 **14** 15	5	1
		69	22

I have added in this table two columns, ſhewing the number of changes in each month, and the number out of each agreeing with the Moon.

I ſhall only add, that no concluſion muſt be drawn from the obſervations of a ſingle year.

XVII. *Extract of a Meteorological Journal for the Year 1774, kept at* Briſtol, *by* Samuel Farr, *M. D.*

Redde, March 23, 1775.

Months.	Barometer.				Rain.
	Higheſt	Loweſt.	Mean.	Viciſſitude.	
January	30,1	28,8	29,5	1 1-2	4,951
February	30,4	29,2	29,7	0 9-1	5,549
March	30,2	29,1	29,7	0 9-4	5,297
April	30,1	29,3	29,7	0 8-5	2,349
May	30,1	29,3	29,9	0 7-4	2,955
June	30,2	29,4	29,7	0 6-3	2,602
July	30,2	29,7	29,8	0 4-1	2,972
Auguſt	30,2	29,4	29,8	0 5-2	2,999
September	30,1	29,0	29,6	0 7-2	7,035
October	30,5	29,3	30,0	0 8-2	1,927
November	30,2	29,2	29,7	0 6-1	1,683
December	30,6	29,0	29,7½	0 7-2	2,047
					42,366

The barometer was placed ſeventeen yards above the level of the river Avon, which runs very near to my houſe. By viciſſitude is meant the greateſt riſe or fall of the quickſilver in the ſmalleſt number of days.

S. FARR.

Dr. FARR had alſo given the mean heights of the thermometer within doors for every month in the year. But theſe are omitted, becauſe obſervations of the thermometer in the houſe are of no importance, unleſs accompanied with correſponding ones of an inſtrument kept in the ſhade in the open air. The air of a room, though kept without a fire, and ſo ſituated as never to ſee the Sun, alters its degree of heat or cold ſo much more ſlowly than the external air, that no judgement can be formed of the temperature of the one from that of the other: except after a continuance of weather of the ſame kind for a long time together, their mutual relation is vague and undetermined. Dr. FARR likewiſe ſent a particular account of the winds and changes of the weather for every day of the year; from which I have compoſed the two following tables.

S. HORSLEY.

An abridged TABLE of the WINDS for BRISTOL, for the Year 1774.

	N	S	E	W	NW	SE	NE	SW			Number of Froſty Days.
January	3½	½	6	3	1½	2	7	7½	31	3 days in the year are omitted in Dr. FARR's account; *viz.* Feb. 7. April 29. and July 12.	10
February	½	1½	½	1	3½	3	5½	11½	27		7
March	½	1½	4½	½	3½	5½	11	4	31		7
April	½	2	½	0	8	4½	5	8½	29		
May	½	1½	2	0	2	2	14½	8½	31		
June	1	2½	2	½	4	1	2½	16½	30		
July	1	1	0	2	6½	2	0	17½	30		
Auguſt	0	½	1½	0	1	4	6½	17½	31		
September	½	½	0	½	4	10	7½	7	30		
October	0	1	2	½	3½	6	5½	12½	31		Froſt at times.
November	1	½	0	0	4	5	13½	6	30		Froſty nights.
December	0	0	3	0	½	8	13½	6	31		18
	9	13	22	8	42	53	92	123			42

Thunder. February 16. 23. 24. S.W.
March 8. 20. E. 28. E. and W. 30. E. and N.E.
April 27. with hail ſtorm, S.W. and N.W.
May 1. 4. N.E. 9. 10. E. 24. S.W. and S.E.
June 25. S.W. and S.
July 10. S.W. 26. N. and N.W.
September 4. S.E. and N.E. 6. N.W. 12. S.W. and S.E.

TABLE for Trial of the Moon's Influence at BRISTOL, for the Year 1774.

	Laſt Qr.	New.	1ſt Qr.	Full.			
	D. H.	D. H.	D. H.	D. H.			
Jan.	5 6	11 21	19 3	27 7	$\overset{-}{1}$ $\overset{\sim}{5}$ $\overset{-}{10}$ $\overset{*}{\overset{\sim}{13}}$ $\overset{-}{19}$ $\overset{\approx}{22}$ $\overset{*}{\overset{-}{30}}$	7	2
Feb.	3 15	10 9	18 0	25 23	$\overset{\approx}{7}$	1	1
Mar.	4 22	11 22	19 20	27 11	$\overset{\sim}{6}$ $\overset{-}{10}$ $\overset{\approx}{17}$ $\overset{\odot\,+}{20}$ $\overset{\approx}{30}$	5	1
Apr.	3 5	10 12	18 15	25 22	$\overset{\odot}{6}$ $\overset{\sim}{10}$ $\overset{\odot}{13}$	3	1
May	2 12	10 3	18 7	25 5 Laſt Qr. 31 20	$\overset{\approx}{1}$ $\overset{\odot}{10}$ 19	3	1
	New	1ſt Qr.	Full.	Laſt Qr.			
June	8 18	16 19	23 12	30 7	$\overset{\sim}{1}$ $\overset{\odot}{6}$ 13	3	1
July	8 9	16 5	22 19	29 20	$\overset{\approx}{1}$ $\overset{\sim}{10}$ $\overset{\odot}{14}$ $\overset{\sim}{17}$ $\overset{\odot}{19}$ $\overset{\sim}{26}$	6	2
Aug.	7 0	14 12	21 3	28 12	$\overset{\odot}{1}$ $\overset{\sim}{3}$ 7 $\overset{\sim}{25}$	4	3
Sept.	5 14	12 17	19 13	27 7	Only 9 fair days.		
Oct.	5 3	12 0	19 2	27 3	$\overset{\odot}{6}$ $\overset{\sim}{17}$ $\overset{\odot}{25}$ $\overset{\sim}{30}$	4	0
Nov.	3 15	10 7	17 18	25 23	Cloudy with rain till the 5th. hard rain, then frequent froſty nights and gentle rain in the day-time.		
Dec.	3 2	9 17	17 12	25 17	$\overset{-}{3}$ $\overset{\sim}{11}$ 21	3	2
						39	14

When a number appears in this table without any character over it, it is to be underſtood, that the weather was quite unſettled from that day to the next bearing a mark; and when two or more marks are found over the ſame number, all the different kinds of weather, denoted by the ſeveral marks, took place on that day. The ſame is to be underſtood in the tables, p. 177. and p. 193.

This table diſtinguiſhes the changes of weather which fell on the days of true ſyzygie, true quadrature, and true octagonal aſpect. Setting aſide the very changeable months of September and November, there were 39 changes in the remaining 10, fourteen of which happened upon the days ſpecified; which is almoſt 4 more than belong to them on the even chance. Of theſe 14 changes, only four fell upon the day of a new moon, and none at all upon the day of the full.

XVIII. *Extract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1774. *By* Thomas Barker, *Eſquire. Communicated by Sir* John Pringle, *Bart. P. R. S.*

Redde, March 23, 1775.

		Barometer.			Thermometer.						Rain.
					In the Houſe.			Abroad.			
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	29,77	28,32	29,15	42	31½	35	43	20	29	3,308
	Aftern.				43	32	36	46	28	33½	
Feb.	Morn.	30,05	28,49	29,25	46	33½	40	45	22	34½	1,946
	Aftern				46½	35	41	51½	29½	41	
Mar.	Morn.	29,81	28,56	29,30	48½	38	43	44	28½	36	2,728
	Aftern.				51	39	44½	57½	35½	46	
Apr.	Morn.	29,77	28,72	29,24	53	44½	48	52½	32½	42	1,523
	Aftern.				54½	45½	49	62½	37½	51	
May	Morn.	29,67	28,76	29,35	55	48	51½	55½	40	46	3,142
	Aftern.				56½	49	53	69½	45	57	
Jun	Morn.	29,76	28,87	29,33	62	54	59	61	50	55	2,483
	Aftern.				66	55½	60	73½	56	65½	
July	Morn.	29,76	29,10	29,41	63½	57½	60	61	52	56	3,227
	Aftern.				66½	58½	62	76½	61	66	
Aug.	Morn.	29,80	28,80	29,38	68	58	61½	64	47	55½	3,910
	Aftern.				70	60	63½	78½	59	67	
Sept.	Morn.	29,74	28,70	29,28	65	53	56	61	40	49½	8,000
	Aftern.				68½	53½	57½	73	48½	59	
Oct.	Morn.	30,06	28,92	29,64	56½	46	52	51	34	43½	1,156
	Aftern.				57½	46	53½	64½	42	53	
Nov.	Morn.	29,73	28,73	29,36	52½	35½	43	49	28	37	1,530
	Aftern.				52½	36	44	55½	32	41	
Dec.	Morn.	30,21	28,68	29,60	45½	32	39½	44½	20	33½	2,28
	Aftern.				46	32½	40	47	25	38	
											35,235

Wetteſt Months from one to twelve.		
1	Sept.	8,000
2	Aug.—Sept.	11,910
3	July —Sept.	15,137
4	June —Sept.	17,620
5	May —Sept.	20,762
6	April—Sept.	22,285
7	Mar.—Sept.	25,013
8	Feb. —Sept	26,959
9	Jan. —Sept.	30,267
10	Dec. —Sept.	33,164
11	Nov.—Sept.	36,769
12	Oct. —Sept.	39,390

The quantity of rain this year having exceeded any I ever had before, I have added here a new table of the wetteſt months from one to twelve to thoſe I had before given. See vol. LXI. p. 226. In the ſeventeen months from May 1773 to September 1774 there fell 55,890 inches of rain, which is 3,288 inches each month; and in the three years 1772, 1773, and 1774, there came 93,258 inches; that is, 31,046 inches each year. The year began very ſevere with a ſharp froſt, which was not out of the ground for ſeven weeks together, though in the mean time there were ſeveral breaks, attended with great rains or ſnows and floods, and then freezing again. After the froſt it was windy and wet for near a month, till above a week in March; the latter part of that month and all April were more fair, a good ſeed-time; and though with ſome froſty mornings, yet we had in general more mild weather than there has been in ſpring of late years. There was at times a good deal of fine weather in ſummer, yet mixed with a great deal of wet; it was a great graſs year, and a cool ſummer. The hay-time and beginning of harveſt were ſhowery, yet more hindering than hurting; but the latter part of harveſt in September was exceeding bad indeed. No grain could be carried for three weeks together; for it rained every day, and in great quantities. I never meaſured ſo wet a month before. The wheat and oats were chiefly got in before it, and a great deal of barley; yet, as it was a late harveſt, there was a great deal of the barley out, ſome wheat, and almoſt all the beans and peaſe. The wheat through the ſevere and wet winter was all along thin, and much of it mildewed by the wet towards harveſt. The crop of barley was not amiſs, if it could have been all well got; but ſome of it ſuffered by the wet after it was cut. The beans and peaſe were a remarkable great crop till harveſt; but almoſt intirely ſpoiled in it. There was a great deal of winter meat for the cattle this year, plenty of good graſs, a great deal of hay, and fine crops of turneps; but the ſtraw of that corn, which was out in the wet, was ſpoiled. The weather ſettled juſt at the beginning of October, which was a very fine month, almoſt like ſummer; and it was not till then that the harveſt could be finiſhed. The wheat ſeed-time was good, and the reſt of the year favourable upon the whole; though a froſt at the end of November and beginning of December was earlier than uſual, attended with ſnow, and threatened a ſevere winter in moſt parts of Europe; yet it grew mild again afterward, was in general fair, and the ground continued tolerably dry, and a few froſty days concluded the year.

XIX. *An Account of ſome Thermometrical Obſervations, made by Sir* Robert Barker, *F. R. S. at* Allahabad *in the* Eaſt Indies, *in Lat.* 25° 30′ *N. during the Year* 1767, *and alſo during a Voyage from* Madras *to* England, *in the Year* 1774. *Extracted from the original Journal by the Hon.* Henry Cavendiſh, *F. R. S.*

Redde, April 6, 1775.

THE greateſt part of the obſervations at Allahabad were made within doors; ſeveral were made within a tent placed under the ſhade of trees, ſome in the open air in the Sun, and ſome in the open air in the ſhade; but there is no regular ſeries of obſervations in any one place; nor were they made at ſtated times of the day. Though a thermometer kept within doors is but a very indifferent meaſure of the heat of any climate; yet as I have not ſeen any thermometrical obſervations made in that country, except a few during the heats of the ſummer, and printed in the Philoſophical Tranſactions, vol. LVII. page 218. I have ſet down the great and leaſt heights I meet with in each month.

	Leaſt.	Greateſt.		Leaſt.	Greateſt.
January	58	72	July	81	90
February	60	84	Auguſt	80	86
March	62	94	September	78	83
April	79	96	October	72	87
May	72	101	November	52	86
June	81	99	December	51	64

From the 3d of May to the 4th of June incluſive, a thermometer placed within a tent, under the ſhade of trees, was almoſt every day above 100°, and ſeveral times above 109°, once at 112°. The trees under which the tent was placed formed, as I have been informed, a very thick ſhade; ſo that I think theſe heights are more likely to fall ſhort of the true heat of the open air at that time, than to exceed it. The leaſt height I meet with of the thermometer in the open air in the ſhade, is 42°; which it was at twice in the month of January, at 7 A.M. The greateſt heat is on June 9th, at noon, when it was at 114°, the ſky cloudy; the thermometer within doors at the ſame time 95°, which is leſs than it had frequently been in the month of May; ſo that it ſeems likely, that the heat in the open air in May had frequently been above 114°. During the voyage to England, the thermometer was placed in the round-houſe, and was obſerved regularly at eight in the morning, at noon, and at three in the afternoon; the winds and weather are alſo ſet down. The round-houſe, I have been informed, is one of the uppermoſt row of cabbins, and is reckoned the cooleſt and moſt airy part of the ſhip. From February 13. to April 7. between Madras and the Southern tropic, the thermometer was conſtantly between 77° and 86°, and very ſeldom lower than 80°. From thence to April 23, lat. 34° 12′, about 15° E. of the Cape of Good Hope, between 70° and 80°. From thence to May 20, at St. Helena, between 62° and 72°. Thence to Auguſt 2. in lat. 43° 14′ N. between 71° and 80°; and from thence to Auguſt 15, in the Britiſh Channel, between 62° and 70°. At land it is well known, that the heat is uſually conſiderably greater in the middle of the day than in the morning or night; but it appears from theſe obſervations, that in the open ſea, there is ſcarce any ſenſible difference; for in ſettled weather, the difference between the different times of the day was rarely more than 1°, oftener none at all. In unſettled weather there was frequently a difference of 2°, ſometimes 4°, ſcarce ever more; but then there ſeems no connexion between this difference and the time of the day, it being as often colder in the middle of the day than in the morning or evening, as warmer. There is added a regiſter of the thermometer, in the ſoldiers barracks at Allahabad, on June 8, 1769, when from 10 in the morning to 8 in the afternoon it ſtood conſtantly above 100°, in the hotteſt part of the day at 107°, and during the whole night between 99° and 98°.

Sir Robert Barker, at my requeſt, has been ſo good as to add the following account of the general ſtate of the weather in Bengal.

THE rains at Bengal generally ſet in between the 1ſt and 15th of June, and continue till the middle of October, when it remains fair till February, the wind blowing moſtly from the N.E. quarter, in which month and March it is interrupted by the N.W. ſqualls, attended with violent guſts of wind, thunder, and lightning, with ſhort, but exceſſive hard, ſhowers of rain or hail, commonly one, but rarely two, in each day. From the middle of March to the middle of June the weather is very hot. At Allahabad and the upper country the rains are not expected till the 20th of June, and ſeldom exceed the 30th, excepting in extraordinary ſeaſons, when it has been known to keep off till the 5th of July; but ſuch an event is uſually attended with a great mortality both of men and beaſts. They break up about the middle of September, and from this time to the beginning of January it continues fair cold weather. In January there are, almoſt always, a few days rain, ſeldom more than a week, and that gentle and pleaſant, which is productive of a ſecond crop, which they uſually reap. The winds at Allahabad ſet in Eaſterly from the beginning of the rains, and blow almoſt conſtantly from that quarter until the concluſion of the cold weather in March, when it changes more Northerly, and is attended by violent North-weſt ſqualls of thunder, lightning, rain, and hail, at which time it changes to the Weſt, blowing with violence, and a heat which frequently deſtroys the birds and beaſts in the fields, till the rains afford a relief.

lief. The river Ganges begins to ſwell before the commencement of the rains, reported by the natives to proceed from the melting of the ſnow on the Northern mountains during the heats of May and June. But the ſudden riſe of the waters in the Ganges, a few days after the ſetting in of the rains, is almoſt incredible; ſince it has been known to riſe twenty feet in forty-eight hours; and its ſudden fall is as extraordinary. In Bengal the rivers are of courſe affected by the riſe and fall of the Ganges. Floods continue the whole time of the rains, more or leſs; but the greateſt overflowings are generally at the beginning and the end or the breaking up of the rains, at which period it rains with the greateſt violence. The waters at Allahabad and in all the upper countries run off into the rivers as ſoon as the rain has ceaſed, the ſoil being for the moſt part of ſand, and the country interſected with ſmall rivulets; but in Bengal, and particularly ſo low down as Calcutta, being of a clay ſoil and an extenſive flat, the whole country is overflowed, forming lakes of great extent, ſome of them being ſix miles over. The water, therefore, generally remains till the Sun has exhaled it, by which it becomes putrid, and renders thoſe parts extremely unwholeſome, occaſioning thoſe deadly putrid fevers, which carries off the patient in a few hours, known by the name of *pucker fevers*.

XVI. *A Second Eſſay on the Natural Hiſtory of the Sea Anemonies. By the Abbé* Dicquemare, *Member of ſeveral Academies, and Profeſſor of Natural Philoſophy, &c. at* Havre de Grace. *Tranſlated from the* French.

Redde, Mar. 9, 1775.

I WAS concluding my eſſay on the ſea anemonies inſerted in the LXIIId volume of the Philoſophical Tranſactions, when I diſcovered a fourth ſpecies of that animal, of which I could not at that time take a drawing; and I have reaſon to think, that I have ſince obſerved a fifth ſpecies. New obſer-

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Second Mémoire pour ſervir à l'Hiſtoire des Anémones de Mer. Par M. L'Abbé DICQUEMARE, *de pluſieurs Académies Royales des Sciences, Belles-Lettres et Arts, Profeſſeur de Phyſique Expérimentale, &c. au* Havre de Grace.

Le Havre, 19 Aouſt 1774.

LORS que je terminois le mémoire inſéré dans le LXIII^{me} volume des Tranſactions Philoſophiques, je découvris une 4eme eſpece d'anémones de mer, dont je ne pus donner la figure. Je crois même en avoir depuis apperçu une 5^{me}.

obſervations have encreaſed the number of my experiments: my ideas have been enlarged, my views extended; and the phænomena crowd in ſo faſt upon me that I dare not flatter myſelf with the hopes of ever arriving at the end of this purſuit. The ſcarcity of high tides, the viciſſitudes of ſeaſons, and other ſimilar impediments, make it leſs wonderful that a ſeries of years ſhould often elapſe before it is poſſible to preſent the curious with any diſcoveries of which they might avail themſelves, either by analyſis, combination, or analogy, and thereby furniſh general views and a chain of ideas leading to a new field of diſcovery, the uſual effect of contemplation. I ſhall here endeavour as briefly as poſſible, to communicate ſome of the ideas that have been ſuggeſted to me by my laſt experiments.

How many are the animal functions, which ſeem to depend upon ſenſibility and irritability; and yet how little are theſe faculties underſtood? how ignorant are we of

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5eme. De nouvelles obſervations ont multiplié mes expériences, mes vues ſe ſont étendues, mes deſſeins ſe ſont accumulés, et les phénomenes ſe ſuccedent de maniere à me faire croire que j'en verrai difficilement la fin. La rareté des grandes marées, la viciſſitude des ſaiſons amene des années, des ſuites d'années, avant de pouvoir offrir aux ſavans quelques découvertes dont on puiſſe tirer avantage par l'analyſe, la combinaiſon, l'analogie; et ouvrir par des vues générales, par la liaiſon des idées, &c. de nouvelles routes, fruits ordinaires de la contemplation. Eſſayons ſans prolixité d'expoſer ici quelques unes des vues que m'ont fait naître mes dernieres expériences.

De combien de fonctions ces deux facultés trop peu connues, la ſenſibilité et l'irritabilité, ne paroiſſent-elles pas être la cauſe? mais combien encore la leur nous eſt-elle cachée? Quoique les nerfs ſemblent être chez nous les prin-

their cauſe? The nerves ſeem to be the chief, perhaps the only, organs of ſenſibility in man, and the muſcular fibres to be the principal ſeat of irritability; yet how many are the doubts entertained concerning the parts that are and are not endowed with one and the other! how falſe and erroneous the concluſions relating to the effects they produce, notwithſtanding the many experiments made on animals, whoſe interior ſtructure is the moſt ſimilar to our own! It is then from accurate obſervations on ſuch animals as bear the leaſt reſemblance to our ſpecies that we may hope for new diſcoveries. The ſea anemonies are exceedingly gelatinous, and at the ſame time ſo irritable, that even light affects them, though to all appearance deſtitute of eyes. Might not the rapid and ſingular reproduction of the parts of this animal be attributed to their gelatinous texture? and if ſo, may we not reaſonably conclude, that the reproduction of our vaſcular and fleſhy parts in the conſolidation of wounds is in great

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cipaux, peut-être même les ſeuls organes de la ſenſibilité, et les fibres muſculaires le ſiege de l'irritabilité; que de doutes ſur les parties qui en ſont, ou n'en ſont pas douées! que de fauſſes conſéquences ſur leurs effets, malgré les experiences qu'on a faites ſur les animaux qui ſe rapprochent le plus de l'homme par leur ſtructure intérieure! C'eſt donc en examinant avec une attention ſcrupuleuſe ceux qui s'en éloignent le plus que nous pouvons eſpérer de nouvelles decouvertes. Les anémones de mer, animaux extremement gélatineux, ſont ſi irritables, que la lumiere les affecte, quoiqu'ils ne paroiſſent point avoir des yeux. Une ſubſtance auſſi gélatineuſe ne ſeroit-elle point la cauſe principale de la prompte et ſinguliere reproduction de leurs parties? et ſi cela eſt, ne pourions nous pas penſer que dans la reproduction de nos chairs et de nos vaiſſeaux, dans la conſolidation des playes, &c.

great meaſure owing to ſuch a gelatinous matter; and ſhould we not ſeek for means to increaſe or diminiſh the quantity of that matter as circumſtances may require?—I cannot quit this ſubject without reluctance.—If it be true, that earth and a gelatinous ſubſtance be the conſtituent parts of the muſcular fibres of ſuch animals as we are beſt acquainted with, and that only the latter be capable of irritation; doth it not follow, that the gelatinous nature of the ſea-anemonies is the true cauſe of the effect produced upon them by the impreſſion of light? and may we not conjecture, from the very gelatinous nature of theſe animals, and from their being affected by light on every part of their bodies, but more particularly on thoſe that are recently cut; may we not, I ſay, from hence conjecture, that the gelatinous part of the muſcular fibre is the only one capable of irritation in ourſelves? Might not theſe animals, by a ſober uſe of analogy, or by new experiments, lead us to a more

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la partie gélatineuſe joue un grand role, et chercher les moyens de l'augmenter ou de la diminuer ſelon les circonſtances. J'ai peine à abandonner cet objet. S'il eſt vrai que dans les animaux qui nous ſont plus connus, les fibres muſculaires ſont compoſées d'une partie terreuſe et d'une partie gélatineuſe, et que cette derniere ſoit ſeule irritable, l'effet que produit la lumiere ſur les anémones de mer n'auroit-il point pour cauſe leur ſubſtance gélatineuſe? ou ne pourrions nous pas conjecturer (de ce que ces animaux ſont très gélatineux et affectés de la lumiere par toutes les parties de leur corps, plus même par celles qui ſont récemment coupées) que c'eſt réellement chez nous la partie gélatineuſe de nos fibres muſculaires qui eſt la ſeule irritable? Ces animaux ne pourroient-ils point, par une ſorte d'analogie, dont pourtant il ne faut pas abuſer, ou par de nouvelles expériences, nous faire mieux

more perfect knowledge of thoſe ſingular enemies to man, the tape, the hair worm, and the ſea dragon? I ſhall, however, leave theſe reſearches, to which nature ſeems to invite us, and from which uſeful diſcoveries may probably be derived, to thoſe who apply ſolely to phyſiology; and ſhall here content myſelf with laying before the public, the obſervations and experiments to which I have confined my reſearches.

I continued to obſerve the inferior half of a purple anemony of the firſt ſpecies, which I had cut in two on the 12th of July 1772, and which was alive on the 8th of April 1773, the day on which I concluded my former eſſay: it appeared to me to be daily recovering ſtrength. On the 26th I found it at the bottom of the veſſel. On the 1ſt of July it climbed up the ſides almoſt to the ſurface of the water; and this it repeated on the 15th and 22d, above a twelvemonth after the time it had been cut. On the 25th a crab (*cancer lanoſus, cancer venenatus*) half dried,

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mieux connoitre ces ſinguliers ennemis de les crinons, l'homme les draconcules, le ténia et autres? c'eſt à ceux qui s'occupent uniquement de la phyſiologie à ſuivre ce ſentier, que la nature nous préſente, et qui pourroit conduire à un champ fertile. Reſtreint à mes petites obſervations, et à mes expériences, je me contenterai de les faire paſſer ſous les yeux du public.

La moitié inférieure du corps d'une anémone pourpre de la premiere eſpece, que j'avois coupée le 12 Juillet 1772, et qui avoit vécu juſqu'au 8 Avril 1773, jour où je terminois mon premier mémoire, continua d'être obſervée; elle ſe fortifioit de jour en jour. Le 26 je la trouvai au fond du vaſe, le premier Juillet elle monta le long des parois preſque à fleur d'eau, le 15 et le 22, c'eſt-à-dire plus d'un an après qu'elle eut été coupée, elle y monta encore. Le 25, un cancre (*cancer lanoſus, cancer venenatus*) à demi deſſéché tomba dans le vaſe, y ſéjourna quelques

dried, fell into the veſſel, and after continuing in it ſome hours, infected and tinged the water in the ſame manner as if huſks of walnut or pieces of ſoot had been thrown into it, which had ſuch an effect upon the piece of anemony, that it threw up a great quantity of its inteſtines. On the 30th it laid hold of the ſide again, but was conſiderably ſhrunk. In the beginning of September it received a ſecond injury, from another piece of anemony, which having been damaged in the ſame manner by the former accident, ſuddenly putrified and infected the water: more of the inteſtines were now diſcharged; and this laſt accident, added to the former one, affected the creature to ſuch a degree, that it waſted gradually till the 8th of October, when it was totally diſſolved. The ſea-anemonies are undoubtedly ſuſceptible of irritation to a very great degree; but is all that hath been deſcribed to be conſidered as the mere effect of irritability? Allowing that to be the caſe, will it not follow, that we are more in the dark concerning that faculty than is

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quelques heures, empoiſonna et teignit l'eau, comme l'auroit fait le brou de noix ou un morceau de ſuie: la partie d'anémone en ſoufrit, juſqu'à jetter déhors une quantité d'inteſtins. Le 30 elle s'attacha encore, elle étoit alors diminuée de volume. Ayant eu à ſoufrir dans les premiers jours de Septembre d'une autre partie d'anémone, qui avoit eſſuyé le même accident, et qui ſe corrompant tout à coup gâta l'eau, quelques inteſtins lui ſortirent de nouveau; ce dernier accident ſuite du premier l'affecta ſi fort, qu'elle diminua de plus en plus juſqu'au 8 Octobre, qu'elle étoit totalement décompoſée. Les anémones de mer ſont ſans doute très irritables; mais peut-on rapporter tout ce qui vient d'être expoſé au ſeul effet de l'irritabilité? et ſi cela étoit, cette faculté ne nous ſeroit-elle pas encore moins connue

is generally thought? It is uſual to aſcribe to it the palpitation that is perceived in the fleſh of oxen, when cut from the animal, in the ſevered pieces and hearts of ſome reptiles, as the ſloth, and other involuntary ſpaſmodic motions; but is it poſſible, that determinate motions, that actions which ſeem to imply will, ſuch as clinging, &c. which in our experiment were continued for the ſpace of fifteen months, and, but for an accident, might probably have been carried on much longer, ſhould ariſe from meer irritability without any other cauſe? The upper part of another ſea-anemony, of which the inferior was become a perfect animal, lived ſix months after its being cut, and ſeemed to feed by ſuction upon pieces of muſcle I put in its way. Sea-anemonies, which I had cut diametrically and perpendicularly, were not eſſentially hurt by that operation; which might be expected to diſorder more than any the whole animal oeconomy, and to be particularly injurious to the baſis of this animal, which

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connue que nous ne le penſons? On y rapporte comunément les mouvemens qu'on apperçoit dans la chair de boeuf ſéparée de l'animal, aux tronçons, au coeur de certains reptiles, à celui des pareſſeux, &c. mais des mouvemens déterminés, des actions, comme celle de s'attacher, et autres qui dans cette expérience ont duré quinze mois, et qui auroient été plus loin, peuvent-ils n'avoir que l'irritabilité pour cauſe? La partie ſupérieure d'une autre anémone, dont l'inférieure étoit devenue un animal entier, a vécu ſix mois, et paroiſſoit ſe nourrir en ſuçant des morceaux de moule que je lui préſentois. Des anémones de mer coupées diamétralement et perpendiculairement ont ſoutenu fort bien cette opération, qui ſemble devoir déranger plus que toute autre l'œconomie animale, et offenſer conſidérablement la baſe, qui eſt dans ces animaux une partie très eſſentielle, et dans quelques

which is its moſt eſſential part, and in ſome ſpecies is exceedingly tender. The two ſides ſoon came together, but but were ſome time in contact before they connected. The junction, however, was at laſt ſo perfect, that no viſible ſcar remained upon the robe, the continuity of the little blue edge was not in the leaſt interrupted, and the mouth was perfectly reſtored. Theſe ſemi-anemonies, which I ſtill preſerve, have long ſince acquired the appearance of the perfect animal, and perform all its functions, ſuch as moving from place to place, ſwallowing, &c. This leads to the reflection that if, as hath been aſſerted, the power of loco-motion in theſe animals depends upon a certain combination of ſtraight and circular tubes, it is not requiſite, in order to exert it, that the continuity of theſe tubes be uninterrupted, ſince half an anemony newly cut changes its place with as much eaſe as a whole one. It will no doubt appear a curious enquiry, whether theſe ſemi-anemonies, after becoming in a manner whole ones, are capable of propagating their ſpecies.

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quelques eſpeces, très délicate. Les deux cotés ſe ſont rapprochés; mais ils ont été long temps à ſe joindre ſolidement; cependant cette jonction fut opérée au point qu'il ne reſtoit aucune marque ſur la robe. La petite bordure bleue n'étoit nullement interrompue, et la bouche s'eſt reformée. Ces moitiés d'anémones que je conſerve, et qui ont depuis longtemps tout l'air d'un animal entier, en font toutes les fonctions, comme de changer de lieu, d'avaler, &c. Sur quoi il faut remarquer que s'il eſt vrai, comme on la cru, que ces animaux changent de lieu au moyen de canaux droits et de canaux circulaires, du moins il eſt vrai auſſi qu'il n'eſt pas néceſſaire que ces canaux ſoient entiers, puiſque des moitiés d'anémones nouvellement coupées ne laiſſent pas de changer de place auſſi facilement que l'animal entier. On feroit ſans doute curieux de ſavoir ſi ces moitiés d'anémones, devenues

ſpecies. To this I can only anſwer at preſent, that I have not yet ſeen the generation of anemonies, except in the ſea, or in animals newly taken out of it. It muſt further be obſerved, that theſe anemonies, perfect as they ſeem to be, may perhaps have only half the number of limbs of the whole ones, whereof they made a part: ſo that the whole wonder comes to this; that the ſevered halves of an animal ſhould recover, and each taking the appearance of an entire individual, continue to live as if they were ſuch. And ſuch in fact I believe they are; but this I have not yet been able to aſcertain, as the anemonies of this ſpecies have not all the ſame number of limbs, as it is always very difficult to count them, and as all thoſe upon which I have hitherto made the experiment had a great number of them. However, as no manner of difference appears, I am inclined to ſuſpect that new limbs ſhoot out between the old ones.

After

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devenues en quelque ſorte des animaux entiers, donnent des petits; il ſuffit de répondre ici que je n'ai jamais vu naître des petits que des anémones qui étoient en mer, ou de celles qui en étoient tirées depuis peu de temps. Il faut encore obſerver, que ces prétendus animaux complets n'ont peut-être que la moitié des membres qu'avoit l'animal dont ils faiſoient partie, et qu'ainſi le merveilleux de cette opération ſe réduiroit à voir chaque moitié d'un animal ſe guérir, prendre la forme de l'animal entier, et vivre comme s'il l'étoit. Je crois qu'ils le ſont en effet; mais je n'ai pu encore le vérifier exactement, parceque les anémones de cette eſpece n'ont pas toutes le même nombre de membres, qu'il eſt très difficile de les compter, et que toutes celles que j'ai opérées en avoient beaucoup. L'apparence eſt abſolument la même, peut-être leur pouſſe-t'il des membres entre les autres.

On

After having obſerved theſe animals during ſeveral years, both in the ſea and in my ſtudy, it will no doubt be expected, that I ſhould now give a particular account of their manner of propagation; but here I can only confeſs my ignorance, having never been able to get at the knowledge of any one circumſtance relating to it: which makes me ſuſpect, that theſe animals propagate without any communication of individuals. What I would here ſuppoſe is by no means unexampled. Among the *aphides*, for inſtance, (whoſe mode of propagation deſerves to be further examined) though the ſexual parts have been diſcovered, individuals nevertheleſs are found, which, though deprived of all communication one with another from the very moment they are brought forth, yet produce an offspring, which being likewiſe denied all intercourſe, ſtill propagates; and ſo on, through a great number of generations, which ſucceed each other very rapidly. The muſcle alſo is thought to be an animal of the ſame nature.

The

On s'attend peut-être qu'après avoir obſervé ces animaux pendant pluſieurs années à la mer et dans le cabinet, je vais expoſer ici tout cequi concerne leur génération: point du tout! il ne m'a pas été poſſible d'en découvrir la moindre choſe; ce qui m'a porté à croire, que cette eſpece ſe multiplie ſans qu'un individu ait beſoin de la participation d'un autre. Ce que j'établis n'eſt pas ſans exemple. Les pucerons, dont la génération mérite encore d'être examinée, quoiqu'on y ait découvert des parties ſexuelles, offrent, à ce qu'on aſſure, des individus qui ſéparés les uns des autres dès la ſortie de la mere, donnent des petits, et ceux-ci privés de même de toute communication en donnent encore, ce qui a été pouſſé juſqu'à un grand nombre de générations, parcequ'elles ſe ſuccedent promptement: mais pour ne pas ſortir des eaux; la moule paroit dans le cas que nous ſuppoſons.

La

The anemonies of the ſecond ſpecies are not only leſs obvious to our obſervation, but it is with difficulty they are preſerved in any degree of perfection. They cannot be taken out of the ſand without depriving them of their natural poſition. Common mixed ſand kills them in a few days; and that which is purified affords them no longer the ſlime, the ſmall inſects, or other neceſſary ſuſtenance, which we cannot poſſibly divine. In plucking them from their native ſoil their baſes generally ſuffer, and the wounds in that part are frequently mortal. One of the ſafeſt expedients is to gather with them the pebbles to which they adhere; or what is ſtill preferable, to obſerve them in their natural element the ſea. It is there that, without the leaſt hoſtile appearance, they are ſeen to make an incredible havoc. I have ſeen an anemony of a moderate ſize ſwallow a ſmelt at leaſt ſix inches long. The limbs of this ſpecies, which are much thicker than thoſe of any other, being clipped, new ones ſhoot out as in former caſes. The progreſs of this reproduction,

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La ſeconde eſpece eſt non ſeulement plus difficile à obſerver que la premiere, mais il n'eſt pas facile de la conſerver en bon état. Hors du ſable elle n'eſt plus dans ſa poſition naturelle. Celui qui n'eſt pas purifié la fait périr en peu de jours; celui qui eſt net ne lui fournit plus le limon, ou les petits inſectes, ou quelqu'autre commodité, que nous ne devinons pas. On arrache la baſe preſque toujours, plus ou moins, en la détachant, et ces plaies ſont ſouvent mortelles. Le moyen le plus certain c'eſt d'apporter avec elle, les cailloux ſur leſquels elle eſt attachée, ou, ce qui eſt bien mieux, de l'obſerver à la mer. C'eſt là que ſous l'extérieur le plus paiſible, on lui verra faire un dégat étonnant. J'ai vu telle anémone de moyenne groſſeur avaler un éperlan de ſix pouces de long. Les membres de cette eſpece beaucoup plus groſſe que les autres repouſſent auſſi après avoir été coupés. Les progrès

duction, which is effected in a few days, is ſcarce perceptible; and it is ſo perfect, that no protuberant rim or viſible ſcar remains. Neither the colour, the ſize, nor the form, are any ways altered. This anemony is able to creep when deprived of its limbs; which ſeems to prove, that the communication, which is thought to exiſt between the limbs and the hollow muſcles, may be interrupted, without ſenſibly reſtraining the animal's locomotive powers. Thoſe limbs, it is true, enable the animal to crawl when turned on its back; but do by no means ſerve as legs for walking ſteadily, as hath been erroneouſly aſſerted, and miſrepreſented in ill-drawn figures. I made large inciſions on ſeveral anemonies in the ſea, which healed in a very ſhort time; but I always took care not to injure the baſis, as any conſiderable wound, and eſpecially the leaſt rent, on that part of this ſpecies proves often mortal. I do not mean to queſtion the poſſibility of what hath been repeatedly ſaid of an

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grès de cette reproduction, qui s'opère en peu de jours, ſont difficiles à ſaiſir et à ſuivre, elle eſt ſi parfaite qu'il n'y a aucun bourrelet. La couleur, la groſſeur, et la forme ſont les mêmes. Lorſque l'anémone a les membres coupés, elle ne laiſſe pas de marcher: ce qui prouve que ſi, comme on la cru, il y a communication entre les membres et les muſcles creux, elle peut être interceptée ſans que les mouvemens en ſoient ſenſiblement gênés. Ces membres ſervent bien à l'anémone à ſe trainer, lorſqu'elle eſt retournée ou renverſée; mais ils ne lui ſervent pas de jambes pour marcher ferme, comme on l'a prétendu, et comme le repreſentent des figures mal deſſinées. J'ai fait de grandes inciſions à des anémones qui étoient en mer; elles ſe ſont cicatriſées; mais j'épargnois la baſe: car les bleſſures conſiderables y ſont ſouvent mortelles dans cette eſpece, ſur tout les déchiremens. Ce qu'on a dit et répété d'une anémone, qui ne pouvant rendre une moule, l'avoit fait

an anemony, which not being able to void a muſcle it had ſwallowed, forced it out through a rent it made with the muſcle itſelf at its baſis, and that this rent was ſoon after perfectly cicatrized. But a love of the marvellous too plainly appears throughout the whole narration, and the inferences drawn from the fact give room to ſuſpect, that little attention had been paid to the concomitant circumſtances. Wounds of this nature often occaſion a diſorder in the interior part of the anemony, the progreſs of which ſoon brings on its total diſſolution. Of all the kinds of ſea-anemonies I ſhould prefer this for the table: being boiled ſome time in ſea-water they acquire a firm and palatable conſiſtence, and may then be eaten with any kind of ſauce. They are of an inviting appearance, of a light ſhivering texture, and of a ſoft white and reddiſh hue. Their ſmell is not unlike that of a warm crab or lobſter. I have ſeen ſome of the young of this ſpecies, but have been able to make no diſcovery concerning their mode of propagation.

A de-

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fait ſortir en faiſant avec cette moule une déchirure à ſa baſe, laquelle s'étoit cicatriſée peu après, n'eſt pas impoſſible: mais l'amour du merveilleux s'apperçoit dans la maniere de rapporter le fait, et les conſéquences qu'on en tire font voir qu'on avoit peu obſervé avant et après. Ces ſortes de playes laiſſent ſouvent dans l'intérieur une maladie dont le progrès fait périr l'anémone. De toutes les eſpeces d'anémones de mer, celle-ci m'a paru devoir meriter la préférence pour la table. Lorſqu'on les a fait bouillir un peu longtemps dans l'eau de mer, afin qu'elles ſoient bien cuites et un peu fermes, et qu'on les ſert pour manger à quelle ſauſſe on juge àpropos, elles ont un air ragoutant, une conſiſtence légére et tremblante, une couleur douce mêlée de blanc et d'un rouge agréable, et une odeur d'écreviſſe ou autres cancres encore chauds. Je n'ai rien pu découvrir ſur la génération de cette eſpece: j'ai vu ſeulement de jeunes individus.

Le

A detail of my former obſervations and experiments would be here an uſeleſs repetition: I ſhall therefore only obſerve, that the ſemi-anemonies of the third ſpecies have ſo entirely recovered the parts they had been deprived of, whether the ſuperior or the baſis, that no manner of difference could be perceived. Some of the men of learning, whom my firſt diſcoveries brought to my ſtudy, imagined that the baſis was the moſt eſſential part of the animal, and that the mouth and limbs were to be conſidered only as extremities. I was myſelf inclined to adopt that notion, ſeeing that in all the ſpecies abovementioned, the baſis ever gave the greateſt marks of ſenſibility, that the inteſtines are ſituated in that region, &c. but who, on ſeeing the upper part of an anemony producing a new baſis perfectly ſimilar to that which had been ſevered from it, will any longer maintain ſuch an opinion? During the great equinoxial tides, in places from whence the ſea

ſeldom

Le détail de mes obſervations et de mes premieres expériences ſeroit ici une répétition inutile: il ſuffira de dire, que les moitiés d'anémones de la 3^{eme} eſpece ont repouſſé ſi parfaitement ou la baſe ou la partie ſupérieure qui leur manquoit, qu'il n'eſt pas poſſible de s'en appercevoir. Parmi les ſavans que mes premieres découvertes attiroient à mon cabinet, il s'en trouva qui crurent que ce qui conſtitue eſſentiellement l'animal étoit dans la baſe, et que les membres et la bouche devoient en être regardés comme les extrémités. J'étois moi même tenté de le croire, d'autant que la baſe m'a toujours paru plus ſenſible dans toutes les eſpeces, et que c'eſt dans cette région que ſont contenus les inteſtins, &c. mais quelle apparence de reſter dans cette perſuaſion, lorſqu'on voit la partie ſupérieure reformer une baſe tout à fait ſemblable à celle qui a été retranchée? Pendant les grandes mers d'équinoxe,

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ſeldom recedes, I ſaw ſeveral of theſe animals which had been cut through the middle, perhaps by ſome crab, by the ſudden colliſion of pebbles, or by ſome other means, which though not unnatural, we may yet not be able to account for. They ſoon began to recover. I ſhould have taken them for a new ſpecies, had not my former experiments pointed out to me the gradual reproduction with which nature, no leſs various than impenetrable in her reſources, kindly indulges them. Are not the accidents which happen to birds, quadrupeds, and even to man, frequently followed by effects, which ſeem intended to convince us, that we lay too great a ſtreſs on the reſources of art, and truſt leſs than we ought to do to nature? Although I could never yet arrive at any certain knowledge concerning the generation of this ſpecies, I ſuſpect that it is different from that of the others. Several of my ſpecimens have ſuddenly let fall to the bottom of

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aux lieux d'où la mer ſe retire rarement, j'ai apperçu pluſieurs de ces animaux qui avoient été coupés par la moitié du corps, peut-être par quelque cancre, par le choc ſubit de quelque caillou, ou par quelque autre moyen même naturel que nous ne pouvons pas prévoir; ils commençoient à ſe rétablir. Je les aurois pris pour une eſpece différente, ſi les expériences que j'avois tentées auparavant ne m'avoient fait connoitre le rétabliſſement gradué que leur accorde la nature, dont les reſſources ſont d'autant plus inconnues qu'elles ſont ſans nombre. Ne voit-on pas de temps en temps, à l'occaſion des accidens qui arrivent aux oiſeaux, aux quadrupèdes, et à l'homme même, des effets qui ſemblent deſtinés à nous convaincre, que nous comptons trop ſur les reſſources de l'art, et trop peu ſur celles de la nature. Quoique je n'aye eu aucun éclairciſſement ſur la génération de cette eſpece, je ſoupçonne qu'elle eſt différente des autres. Pluſieurs individus ont jetté tout

of the veſſel in which they were kept, a ſlimy ſubſtance, nearly of the colour of their bodies, perhaps ſomewhat yellower, which, in the microſcope, appeared to conſiſt of a great number of globular particles, pretty much reſembling the ſpawn of fiſh.

The firſt anemonies I procured of the fourth ſpecies, had probably been brought near the coaſt by fiſhermen, for they generally keep in deep water where they are found adhering to oyſter-ſhells. I cauſed ſeveral to be brought into my ſtudy, where being put into ſea-water they ſoon expanded. The largeſt, which opened firſt, puzzled me not a little. I could diſcover no baſis, but only ſaw limbs projecting on every ſide. I flattered myſelf that a greater expanſion would clear up the difficulty; on the contrary it only added to it. The others opened, and appeared in a ſhape much more ſimilar to that which I expected. I ſaw a baſis, a body, a great

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tout à coup au fond du vaſe où je les conſervois une eſpece de limon, de la couleur de leur corps, ou un peu plus jaunâtre, lequel examiné au microſcope paroit formé d'une très grande quantité de globules qui auroient aſſez l'air d'un fray.

Les premieres anémones de la quatrieme eſpece que je trouvai avoient vraiſemblablement été apportées au bord de la mer par les pêcheurs: car elles ſe tiennent au large, et ſont attachées ſur les huitres. J'en fis apporter pluſieurs à mon cabinet et les ayant miſes dans l'eau de mer, elles s'ouvrirent. La plus groſſe, qui s'ouvrit la premiere, me donna bien de la tablature. Je ne ſavois où en pouvoit être la baſe, appercevant des membres de tous cotés; j'attendois à être inſtruit par un plus grand épanouiſſement: mais plus il avançoit moins je voyois clair. Les autres individus s'ouvrirent, et ſe préſenterent ſous une forme plus approchante de celle que j'attendois. Je vis une baſe, un corps, une grande quantité de membres très

great number of ſlender limbs, the aſſemblage of which formed, at firſt, different kinds of tufts, and afterwards various fine plumes of a whitiſh hue inclining to carnation. I returned to my firſt ſpecimen, which now appeared to conſiſt of two animals joined at the baſis. I became very ſolicitous to unravel the myſtery of this ſingular union. At length I perceived, that this was a monſter of its kind, conſiſting of three different animals blended into one. It periſhed twelve days after I had received it. Its internal ſtructure, though in great confuſion, was yet an intereſting object to thoſe who are acquainted with that part of this animal, and who have a taſte for comparative anatomy. It appeared in ſuch diſorder that I can ſcarcely conceive how it was poſſible for the creature to live in that condition. The ſtate of diſſolution, which began ſoon after, and the impoſſibility of repreſenting every part at one view, prevented my taking a drawing of this remarkable inſide. Its mouths

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très déliés dont l'aſſemblage formoit des eſpeces de houpes, enſuite de beaux panaches, le tout blanc tirant un peu ſur la couleur de chair. Je retournai à mon premier individu, qui me parut double, les baſes étoient réunies. Quelle eſt cette union ſinguliere, me diſois-je a moi même. Enfin je m'apperçus que c'étoit une anémone monſtrueuſe, dans laquelle trois individus ſembloient être confondus. Douze jours après, cette anémone a péri. Sa ſtructure intérieure, quoique très confuſe, ne laiſſoit pas d'être intéreſſante à voir, pour quiconque connoit celle de ces animaux, et a du goût pour l'anatomie comparée. Elle étoit dans un boulverſement ſi grand que j'ai peine à comprendre, comment l'animal avoit vécu. Il ne m'a pas été poſſible de deſſiner cet intérieur à cauſe de l'état de décompoſition qui commençoit, dailleurs il falloit voir partie après partie, ce qu'il n'étoit pas aiſé de bien rendre. Ses bouches de coté et d'autre étoient bien faites,

mouths on either ſide were regularly ſhaped, but rather leſs than the uſual ſize; and in the folds, formed by the baſes, ſeveral limbs appeared, which ſeemed to belong to a third animal, incorporated in the two that were more apparent. The ſequel will ſhew, that this is not the only peculiarity I have obſerved in this ſpecies, which by its manner of propagating ſeems particularly calculated for producing monſters. The anemonies of this kind are commonly found adhering to the convex ſhells of oyſters: they abound in the road of Havre de Grace, ſo that I had no difficulty in procuring whatever number I choſe. A viſcoſe matter, like that which is ſeen on fiſh newly caught, iſſues from them. I have opened between two or three hundred of a large ſize, but I never found in any of them either whole or parts of animals; and yet as often as I offered them a piece of oyſter or muſcle, they would ſwallow it. The large anemonies of this ſpecies are generally ſurrounded by a multitude of ſmall and middle-ſized

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faites, ſeulement un peu petites. Dans les duplicatures que formoient les baſes on appercevoit des membres qui paroiſſoient appartenir à un troiſieme individu confondu dans les deux plus apparens. On verra par la ſuite que ce n'eſt pas la ſeule ſingularité que m'ait offerte cette eſpece, qui paroit très propre, par ſa maniere de ſe perpétuer, à donner des productions monſtrueuſes. C'eſt ordinairement ſur l'écaille la plus convexe des huitres que ces anémones ſe trouvent attachées: il y en a dans les rades du Havre une quantité prodigieuſe; il ne m'a donc pas été difficile de m'en procurer. Elles rendent une matiere viſqueuſe comme celle du poiſſon nouvellement pêché. Quoique j'en aye ouvert de deux à trois cens fort groſſes, je n'ai jamais trouvé dedans aucun animal ou portion d'animal: mais lorſque je leur ai préſenté des morceaux d'huitres ou de moules, elles les ont avalés. Autour d'une groſſe anémone de cette eſpéce on en voit ordinairement

une

ones, which form very pleaſing groups (ſee the figure, TAB. VI.). In ſearching into the myſtery of this numerous progeny, I obſerved that the baſes of ſome of theſe ſmall anemonies were not perfectly round, and that in others they mutually adhered one to another: nor was this communication nothing more than apparent; for whenever I ſlightly touched with a pointed inſtrument, the baſis between two connected anemonies, they both contracted at the ſame time. I alſo obſerved, that this common baſis diſtended itſelf gradually near the middle, where it aſſumed the appearance of a net, which burſting at length, left every ſmall anemony to live by itſelf. I moreover perceived, that there iſſues out of the body of the anemonies of this ſpecies, through little pores, and alſo out of their mouths, a conſiderable number of round, ſoft, limber threads, of the thickneſs of a horſe-hair, and of the colour of the animal. On viewing them through a lens, I obſerved a great

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une multitude de petites et de moyennes qui forment de très jolis groupes (voyez la planche). En recherchant tout ce qui pouvoit me dévoiler le miſtère de cette nombreuſe progéniture, je remarquai, qu'il y avoit certaines petites anémones dont la baſe n'etoit point ronde, et d'autres unies entr' elles par la baſe. La communication n'étoit pas ſeulement apparente: car lorſque je touchois légerement avec une pointe fine la baſe entre les deux petites anémones, elles ſe fermoient en même temps; je remarquai de plus, que cette baſe commune s'élargiſſoit peu à peu par le milieu, et y devenoit comme un filet, et qu'enfin elle s'y rompoit, ce qui procure à chaque petite anémone la faculté de vivre en particulier. J'obſervai encore qu'il ſort du corps des anémones de cette eſpece, par de petits trous, et de leur bouche, un nombre conſiderable de cordons ronds, moûs, déliés, de la groſſeur d'un crin, et de la couleur de l'animal: je les examinai à la loupe; ils me parurent

great reſemblance between them and the ſpermatic veſſels of men, when ſtripped of their outward ſheath. Through a common microſcope I ſaw fibres in them which croſſed each other in every direction: and by means of a ſolar microſcope, which magnified them to a diameter of five inches, they appeared of a very cloſe texture, which, when decompoſed, ſeemed to conſiſt of an infinite number of veſſels, croſſing each other in almoſt every direction; but fartheſt extended lengthwiſe. A liquor ſeemed to circulate in the largeſt of theſe veſſels, which, where they meet, form kinds of ganglions like the optic nerves in man. Such an organization cannot ſurely but be intended for valuable purpoſes. Is it not probable, that theſe threads contain certain knots, bulbs, knobs, or buds, which open in time, and cleaving to the bodies on which thoſe threads are extended, produce ſmall anemonies, which at firſt communicate one with another, but afterwards ſeparate by a contraction,

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à peu près figurés comme les vaiſeaux ſpermatiques dans l'homme dépouillés de leur gaine. J'y voyois au microſcope ſimple des fibres croiſées en tout ſens, et au microſcope ſolaire apperçues ſous un diametre de cinq pouces, on voyoit un tiſſu ſerré, qui venant à ſe décompoſer laiſſoit appercevoir une quantité immenſe de vaiſſeaux croiſés preſque en tout ſens, et qui cependant s'étendoient plus ſur la longueur. Une liqueur paroit circuler dans les plus gros, qui en ſe rencontrant forment des eſpeces de ganglions comme les nerfs optiques dans l'homme. Une telle organiſation eſt ſans doute deſtinée à des uſages précieux. N'y auroit-il point dans ces cordons certains nœuds, bulbes, boutons, œuilletons, qui venant à s'ouvrir et à s'attacher ſur les corps où ils ſont étendus, y formeroient autant de petites anémones qui d'abord communiqueroient l'une à l'autre, et ſe ſépareroient en

traction, as I have myſelf obſerved in ſome of them. This I concluded from my having never found any young ones in the great number of anemonies I have opened; at the ſame time that I have ſeen prodigious quantities, of a very ſmall ſize, adhering to oyſter ſhells. This, however, amounted to no more than a conjecture; which not being confirmed by ſubſequent obſervations, I might have been tempted to conclude from analogy, that (as in the caſe of the firſt ſpecies) the young ones are brought forth, ready formed, through the mouth, at certain ſeaſons in which I have not yet opened any of theſe anemonies. Analogy is too frequently miſapplied, and in this inſtance it would have led me into error. A knowledge of the operations of nature is to be acquired by cloſe inſpection, not by looſe conjectures. From a ſeries of obſervations, which to avoid prolixity I ſhall not here repeat, I have learnt, among other ſingularities, that theſe animals having their baſes irregularly diſtended, and their extremities cloſely adhering to ſome hard body, commonly

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enſuite par étranglement, comme j'en ai vu. Je raiſonnois ainſi, n'ayant trouvé aucuns petits dans les anémones que j'avois ouvertes, tandis que j'en voyois prodigieuſement et de très petites ſur les huitres. Ce n'étoit donc qu'une conjecture qui, ne ſe réaliſant pas, auroit pu me laiſſer penſer par analogie que, comme dans la premiere eſpece, les petits naiſſoient tout formés par la bouche, dans certaines ſaiſons où je n'avois pas ouvert de ces anémones. Cette analogie, dont on abuſe ſi ſouvent, m'auroit trompé. Il faut voir, et non pas deviner les opérations de la nature. Des ſuites d'obſervations, que je ne puis raporter ici ſans prolixité, m'ont appris entr'autres ſingularités, que ces animaux ayant la baſe inégalement étendue et fortement attachée par les extrémités ſur un corps dur, ſouvent une

commonly an oyſter ſhell, they, by ſuddenly ſhrinking, leave upon that body ſome ſmall portions of the rim of their baſes, in ſize inferior to a lentil. Theſe little ſhreds have, at firſt, no determined figure; but gradually aſſume the rounded ſhape of a drop of tallow: at length, in the ſpace of two or three months a hole appears in the middle, which forms the mouth. An internal organization, dilatations and contractions, ſenſibility, and other gradual improvements, ſoon after prove them to be animals ſimilar to thoſe to which they owe their origin. It might be imagined, that ſome time muſt elapſe before they can grow to a circumference of two feet. I have not been able to follow them to that degree of increaſe; but I have ſeen them in my houſe, where they are far from being ſo favourably ſituated as in the ſea, growing to a ſize large enough to convince perſons of ever ſo little obſervation, that they belong to the ſpecies of the large anemonies. The ſame ſhred often produces ſeveral

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une groſſe huitre, ils ſe retirent ſur eux mêmes, et laiſſent par ce moyen ſur ce corps une ou pluſieurs petites portions du bord de leur baſe, moins groſſes qu'une lentille. Ces petits lambeaux paroiſſent d'abord informes, ils s'arrondiſſent peu à peu en goute de ſuif; enfin dans l'eſpace de deux à trois mois, on y obſerve un trou dans le milieu, c'eſt la bouche; une organiſation intérieure, des dilatations, des contractions, la ſenſibilité, et autres progrès ſucceſſifs, qui en font une anémone ſemblable à celle dont ils tirent leur origine. Il paroit qu'avant d'avoir acquis deux pieds de circonférence, cela doit être long. Je n'ai pu les ſuivre juſqu'à ce terme: mais je les ai vues chez moi où elles ne ſont pas dans des circonſtances ſemblables à celles de la mer, devenir aſſez grandes pour que les gens les plus groſſiers puſſent les reconnoitre pour appartenir aux grandes anémones. Souvent

ſeveral ſmall anemonies, which at firſt adhere together, and in time are ſeparated by the little contraction I have already mentioned; but if they happen to remain connected, they then produce ſingular forms and often monſters. Beſides the anemony abovementioned, the old one of this ſpecies, which hath particularly unravelled this myſtery to me, was formed in the ſhape of a Y, having two perfect bodies, whereof the baſes, both perforated, adhered and communicated to the ſame trunk; of which I convinced myſelf by obſerving the food which deſcended into the main trunk: neither did theſe two anemonies, thus connected, ever appear to me to have different inclinations, as is the caſe with thoſe that are once ſeparated. Is it not reaſonable therefore to ſuppoſe, that in this ſtate of union, every want was common, and each had its ſeparate deſire of ſatisfying it; and that, to keep up the habitual exerciſe of the animal functions in each, both were upon all occaſions prompted to the performance

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vent pluſieurs petites anémones ſe dévelopent du même lambeau, de ſorte qu'elles ſont adhérentes entr' elles. Peu à peu il s'y forme, comme je l'ai fait connoitre, un petit étranglement qui les ſépare. Quelquefois auſſi elles reſtent unies: alors il en réſulte des ſingularités, ou même des monſtres. Outre celui que j'ai cité, l'anémone mere de cette eſpece, qui m'a dévoilé plus particuliérement le ſecret, étoit formée comme un Y, c'eſt a dire qu'elle avoit deux corps parfaits dont les baſes (cependant percées) étoient adhérentes à une même tige, à laquelle ils communiquoient, ce dont je me ſuis aſſuré, en obſervant la nourriture qui deſcendoit juſques dans le gros tronc: auſſi ces deux anémones ainſi réunies ne m'ont-elles jamais paru avoir deux volontés comme celles qui ne le ſont pas. Chaque anémone n'avoit-elle donc point dans celle-ci d'inclination particuliere à ſatisfaire au beſoin commun? n'étoit-elle point ſollicitée à faire ſes fonctions pour en entretenir

formance of the ſame function at the ſame time? It is to be preſumed, that two ſimilar united quadrupeds would vie with each other in taking the neceſſary nouriſhment, in order thereby to ſatisfy the particular need, which each would have, of keeping up the habit of deglutition, and the dependant functions. (Vide TAB. VI.)

In order to imitate the effects of nature, I clipped ſeveral ſmall pieces from the rim of the baſes of anemonies of this ſpecies, and preſerved them. Some of them became ſmall anemonies, ſimilar to thoſe that had been torn off of their own accord; but many periſhed without producing any thing. May we not conclude from this experiment, that the prolific pieces contained a ſmall bulb intended to become a new anemony, and to be ſoon after torn off by the mother; and that thoſe which periſhed, either contained none of thoſe bulbs, or ſuch as were not ſufficiently formed to thrive and grow after a violent

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tenir l'uſage? Il ſemble que deux quadrupedes réunis et parfaitement égaux, chercheroient tous deux à manger, peut-être à l'envi l'un de l'autre, pour ſatisfaire au beſoin particulier à chacun d'entretenir l'uſage de la déglutition, de tout ce qui en dépend, et de ce qui la ſuit immédiatement (voyez la fig.).

Pour aller au devant des effets naturels, j'ai coupé des petits morceaux des bords de la baſe des anémones de cette eſpece, et je les ai conſervés; pluſieurs ſont devenus de petites anémones, comme celles qui étoient provenues des arrachemens que s'étoient faits les anémones, et un grand nombre a peri ſans produire. Ne pourroit-on pas penſer d'après cette expérience, que ceux de ces morceaux qui ont produit renfermoient un petit bulbe propre à devenir anémone, et deſtiné à être bientôt detaché par la mere; et qu'au contraire les parties qui ont péri n'en contenoient pas, ou n'en renfermoient que de trop peu formés pour ſe déveloper après une

violent amputation? I the rather incline to this opinion, having obſerved, that among the pieces I had cut off, thoſe particularly ſucceeded which appeared interiorly replete and of a certain thickneſs. If ſo, this conjecture may poſſibly lead us to another. It is well known that the freſh water polypi increaſe by ſection, and that being cut into ſeveral pieces, each of theſe pieces becomes an animal ſimilar to the original one. Thus a polypus being divided in 2, 4, 8, 16, or more parts, each of theſe parts probably contains a bulb capable of becoming another polypus. In the courſe of my experiments, the ſmall pieces I cut off from the baſes and robes of the anemonies did not exceed the 500th part of the animal; it is not therefore to be wondered, if many of them did not prove prolific; they probably contained none of the fertile bulbs. The reproduction of the polypus by ſection will then no longer be attributed to any of its rude and ſhapeleſs parts; but rather to

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une ſection violente. Je ſuis porté à le croire, parce qu'en coupant ces morceaux, ceux où je voyois une certaine épaiſſeur, un certain arrondiſſement intérieur, ſont ceux qui m'ont paru réuſſir. S'il en eſt ainſi, cette conjecture pourroit peut-être nous conduire à une autre. On ſait que les polipes d'eau douce ſe multiplient par la ſection, et qu'un tel polipe étant coupé en pluſieurs morceaux, chaque morceau devient un animal ſemblable au premier. On coupe pour cet effet un polipe en 2, 4, 8, 16 parties et plus, chacun des ces morceaux eſt la 16^{eme} partie de l'animal, il ſe peut que dans chaque 16^{eme} partie il ſe trouve un petite bulbe diſpoſé à devenir un polipe. Dans mes expériences, en coupant de petits morceaux du bord de la baſe et de la robe de mes anémones, je ne coupois pas la 500^{eme} partie de l'animal: on ne doit donc pas s'étonner, ſi beaucoup de ces petits morceaux ne produiſent rien; ils ne contiennent peut-être pas de bulbe. Alors la réproduction du polipe par ſa partie ne paroitroit plus due à une portion informe; mais à une

to parts that are organized in a particular manner, to eggs, or perhaps to ſomething more than eggs. The ſingular propagation of ſeveral kinds of this animal ſeems to favour this conjecture. In ſo minute an object as the freſh-water polypus, much is eaſily overlooked; but in the ſea-anemonies, although we are far from ſeeing every thing, yet it is poſſible, even without the aſſiſtance of glaſſes, to diſcern a great deal which muſt eſcape us in the moſt diligent examination of the other animal. Although I ſpeak this from experience, I do not pretend that our ideas concerning reproduction, and eſpecially the reproduction of parts, ſhould be confined to what is here aſſerted. It is known that every part of a living animal, &c. hath an organization far ſurpaſſing any idea we can form of it; that its leaſt particles are ſtill organized; and there is no ſaying how far this may extend. In ſeeds, &c. this organization admits of an aſtoniſhing degree of expanſion,

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partie plus particulierement organiſée, à un oeuf, ou même à quelque choſe de plus qu'un oeuf. La propagation ſinguliere de pluſieurs eſpeces de polipes ſemble favoriſer cette conjecture. Les polipes d'eau douce ſont ſi petits qu'il eſt facile de ne pas tout appercevoir. Dans les anémones de mer, quoiqu'on ſoit fort éloigné de tout voir, on peut cependant, même ſans le ſecours de la loupe, remarquer bien des choſes qui échappent dans l'obſervation du polipe d'eau douce. Quoique je parle ici d'après l'expérience, je ne diſſimulerai pas, qu'on ne doit point y reſtreindre ſes idées à l'égard de la reproduction et ſurtout de la reproduction de parties. On ſait que toutes les parties d'un être vivant, &c. ont une organiſation qui ſurpaſſe infiniment l'idée que nous pouvons nous en faire; que ſes moindres particules ſont encore organiſées quoiqu'infiniment petites; on ne ſait pas même juſqu'où cela peut aller: cette organiſation permet dans les graines, &c. des dévelopemens

expanſion, which doth not always take place in other caſes; but yet often operates when we are the leaſt aware of it. Far then from ſuffering ourſelves to be ſurprized at ſtrange effects in nature, we ſhould in ſome meaſure expect them; we ſhould be conſtantly upon the watch for them, and take pains to be apprized of them, and ſtudy to turn them to the benefit of mankind. Practitioners in ſurgery know how often they have been ſurprized by unexpected events. Such events, which commonly are only the objects of unavailing wonder, would perhaps be leſs unprofitable, if they were cautiouſly obſerved and correctly deſcribed by perſons of knowledge, leſs fond of the marvellous than friends to truth. Lobſters are ſeen to produce new legs when deprived of their old ones; but not exactly at the places where they had been cut off. Their manner of growing is much like to that of our teeth, the interior part and very origin of the claw producing a ſmall one, &c. and this reproduction ſeems not to take place when the lobſter

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mens d'une grandeur étonnante, qui n'ont pas toujours lieu ailleurs: mais qui operent quelquefois au moment qu'on s'y attend le moins. Loin donc de ſe laiſſer ſurprendre par des effets ſinguliers, on devroit en quelque ſorte les attendre, et être très attentif a les ſaiſir, à les connoitre, et à en profiter. Ceux qui s'occupent de l'art de guérir ſavent combien ils ont été ſurpris par des effets inattendus. Ces effets, dont on ſe contente le plus ſouvent de parler avec étonnement, ne ſeroient peut-être pas ſtériles, ſi obſervés avec attention par des gens inſtruits, et auſſi peu avides du merveilleux qu'amis de la verité, ils étoient décrits avec préciſion. Les pieds des écreviſſes repouſſent; mais ils ne repouſſent pas immédiatement de la partie coupée; ils pouſſent à peu près comme nos dents, c'eſt-a dire que de l'intérieur et vers l'origine du pied, on en voit pouſſer un petit, &c. et cette reproduction ne

paroit

is deprived of the phalanx next to the body. There are worms which are known to multiply by ſection; although their ſmallneſs, which renders it very difficult to examine them, and the ſimilarity they bear to other worms, whoſe reproduction we know to be imperfect, may furniſh ſome grounds to doubt, whether this reproduction be ſo compleat as hath been imagined. There is alſo a reproduction in the tails of lizards which cannot well be accounted for. There are experienced naturaliſts who ſtill make a doubt of the multiplication of the polypi by ſection; and others who abſolutely deny it, pretending that what hath been taken for a ſingle animal may as probably be a ſheath, the common work, and habitation of a multitude of ſmall inſects. For my part, far from doubting the poſſibility of ſuch multiplications, of which I have ſeen poſitive inſtances in the ſea-anemonies, I am not for having recourſe to the particular ſuppoſition of any kind of eggs for explaining it. I believe indeed, that propagation is

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paroit pas avoir lieu, lorſque la phalange la plus proche du corps de l'écreviſſe a été enlevée. Certains vers ſe multiplient par la ſection: on doutera peut-être que cette multiplication ſoit auſſi complette qu'elle a paru l'être, parce qu'on n'y voit pas ſi bien à cauſe de leur petiteſſe, et de l'analogie qu'ils peuvent avoir avec d'autres vers dont la reproduction eſt imparfaite. On voit de même une reproduction qui laiſſe quelque choſe à déſirer dans la queue du lézard. Il ſe trouve encore des ſavans qui ſuſpendent leur jugement ſur la multiplication du polipe par la ſection; d'autres la nient abſolument. Ils croyent que ce qu'on a pris pour un individu pourroit être un fourreau, ouvrage et logement de certains petits inſectes. Loin de douter de la poſſibilité de ces multiplications, dont j'ai des exemples déciſifs dans les anémones de mer, je ne les aſſujettis pas même à des eſpeces d'oeufs; je crois ſeulement que c'eſt la voye la plus ordinaire; et les reproductions

is moſt uſually carried on by eggs. But the reproduction of very conſiderable and the moſt eſſential parts of my ſea-anemonies convinces me, that there are wonders far beyond any thing we can imagine, which make a part of the regular eſtabliſhed courſe of things. The ſections of the ſea-anemonies of this ſpecies differ in nothing from thoſe of the former, except that I never yet obſerved an upper half producing a new baſis; the contrary never yet failed. The firſt obſervation of any note, was made on the 16th of June 1773, in the preſence of ſeven ſurgeons whom the ſingular phænomena of reproduction had induced to viſit my ſtudy. The animals of this ſpecies being very large, I only operated upon young ones, and on that day cut one that was not thicker than a gooſe-quill. On the 30th the upper part was perfectly reſtored, and the fold which is ſeen near the upper extremity of the body of this ſpecies, appeared exactly like that I had cut off. By

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productions de parties eſſentielles et très conſiderables de mes anémones de mer m'obligent à croire, que les merveilles, qui font partie de l'ordre établi, vont encore plus loin que notre imagination. Les ſections opérées ſur cette eſpece d'anemones de mer, ont donné à peu près les mêmes réſultats qu'aux autres, ceſt-a dire, que coupées par la moitié du corps, elles ont repouſſé une partie ſupérieure: cependant je n'ai point encore obtenu de baſe aux parties ſupérieures que j'en avois privées. La premiere opération notable ſe fit le 16 Juin 1773, en préſence de ſept chirugiens que les phénomenes de la reproduction avoient attirés à mon cabinet. Comme cette eſpece eſt fort groſſe, je n'avois opéré que de jeunes individus: celui que je coupai alors n'étoit même que de la groſſeur d'une plume à écrire. Le 30 la partie ſupérieure étoit totalement reproduite, et le pli qu'on voit vers l'extrémité ſupérieure du corps dans cette eſpece paroiſſoit exactement comme celui que j'avois retranché. L'exercice m'ayant rendu plus adroit, je ſuis parvenu à couper d'un

By practice I arrived at ſuch dexterity as to cut in two, at one ſnip of the ſhears, in a very ſtraight line, an animal of this ſpecies as thick as my arm. This was performed on the 18th of October: before the end of the month new limbs appeared, of which the large ones, within the tufts, ſhot forth long before the others. On the 10th of December the animal began to eat, though its mouth was ſcarcely formed. The upper part was ſtill alive. I tied a ſtring round ſome of theſe anemonies whilſt they were conſiderably extended length-ways, and pulled the nooſe very tight. They had the dexterity to free themſelves in a few hours of this troubleſome ligament, by gradually withdrawing their upper extremity: then on meaſuring the nooſe I found it not quite ſix lines in diameter. This ſpecies is good to eat.

Among the ſea-anemonies brought me by my fiſhermen, I have ſome reaſon to think that I have diſcovered a fifth ſpecies, which ſeems to reſide only in places from whence the ſea ſeldom recedes. They appeared to me to be

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d'un ſeul coup de ciſeaux et bien droit un de ces animaux gros comme le bras. C'étoit le 18 Octobre; avant la fin du mois il reparut de nouveaux membres. Les gros qui ſont en dedans des panaches paroiſſent longtemps avant les autres. La bouche à peine formée le 10 de Décembre l'animal a mangé. La partie ſupérieure vivoit encore. J'ai lié de ces anémones groſſes comme le bras, lorſquelles étoient fort allongées, et je les ai ſerrées fortement; elles ont eu l'adreſſe de ſe dégager en peu d'heures d'un lien auſſi incommode en dépaſſant peu a peu leur partie ſupérieure. Alors j'ai meſuré le diametre du nœud; il n'avoit pas ſix lignes. Cette eſpece eſt bonne à manger.

Entre les anémones de mer que m'ont apporté mes pêcheurs, je crois en avoir découvert une cinquieme eſpece. Elle paroit réſider dans les lieux que la mer ne découvre jamais. Sur ce que j'en ai vu, elle eſt petite comme la premiere, ſes membres

be as ſmall as thoſe of the firſt ſpecies: their limbs, which are ſomewhat confuſedly arranged in three rows, are alſo nearly the ſame. They have the form and the knobs of the ſecond, and the threads of the fourth ſpecies, which latter, however, are coloured. Their mouths are round, and bordered with ſmall reddiſh limbs; only one white ſpot is ſeen on one ſide of the mouth, whereas two of them appear in thoſe of the third ſpecies. The middle between the mouth, and the limbs is of a greeniſh hue, with narrow variegated ſtreaks which extend from the center to the circumference. The ſpecimens I have ſeen were white on the ſuperior edge of the robe, of a golden yellow in the middle, inclining to a duſkier colour towards the bottom; that is to ſay, the ground-colour of the robe changed gradually from white at the top to brown at the bottom, paſſing, by imperceptible tranſitions, through a ſucceſſion of yellow ſhades, partaking more or leſs of the colour of gold. This whole robe was ſpeckled with light crimſon ſpots, and no rim ap-
peared

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membres ſont à peu près ſemblables, elle en a trois rangs un peu confondus. Elle a de la ſeconde la forme et les mamelons, et les cordons de la 4^{eme}, mais colorés; la bouche eſt ronde bordée de petits membres rougeâtres; une ſeule marque blanche ſe trouve à coté de la bouche, comme on en voit deux dans la 3^{eme} eſpéce. Le milieu entre la bouche et les membres eſt verdâtre, avec de petites rayes de couleur différente, qui vont du centre à la circonférence. Les individus que j'ai vus étoient blancs par le bord ſupérieur de la robe, d'un jaune doré par le milieu, et s'obſcurciſſant vers le bas, c'eſt-à dire, que par des nuances imperceptiblement fondues, plus ou moins dorées, le fond de la couleur de cette robe paſſoit du blanc au brun. Toute cette robe étoit mouchetée d'un beau cramoiſi clair, et il n'y avoit point de
bordure

peared at the baſis. Theſe anemonies had been found on old *volutes*, called ſpindle-ſhells *(fucus brevis)*. Another ſpecimen which was found adhering to an oyſter-ſhell was of a darker colour; but its limbs bore ſome reſemblance to the horns of cattle: they were of a pale green, with circles of a fine dark brown, which had a very pleaſing effect. Theſe limbs appeared, at firſt ſight, to tend towards the center, by the continuation of ſome ſemi-circles which gradually diminiſhed.

I have ſaid little concerning the mechaniſm by which theſe animals perform their different functions, not thinking myſelf ſufficiently acquainted with it to ſatisfy even the moſt chaſtiſed curioſity. I leave to others the cultivation of this field, where the genius of ſyſtem may find an ample range. As in phyſics the only real information is derived from a knowledge of facts, I thought it my duty to confine

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bordure à la baſe. Ces anémones étoient attachées ſur de vieux coquillages à volute, qu'on nomme fuſeaux *(fucus brevis)*. Un autre individu pêché depuis, attaché ſur une huitre, avoit la robe d'une couleur obſcure, mais les membres paroiſſoient comme certaines cornes de boeuf de couleur douce, d'un blanc verdâtre, avec des cercles d'un beau brun noir qui produiſoient un très bel effet. Ces membres ſembloient au premier coup d'œil prolongés vers le centre, par la continuation de quelques demi cercles allant en diminuant.

J'ai dit peu de choſe du méchaniſme par lequel chaque faculté, chaque fonction s'opère dans les anémones de mer, ne pouvant me flàter d'en connoitre aſſez pour ſatisfaire à la curioſité la mieux réglée. C'eſt un champ ouvert à quiconque voudra le cultiver, c'eſt une carriere dans laquelle l'eſprit de ſyſtême pourra prendre ſon eſſort. Comme en matiere de phyſique les connoiſſances tirées des faits ſont les ſeules qui nous éclairent, j'ai cru devoir m'en tenir à la ſimple expoſition

confine myſelf to the bare expoſition of thoſe I have obſerved. Not that I am apprehenſive of being thought too prolix or tedious by thoſe for whom I am writing: I know how much they value every truth, and every new diſcovery: but the habit of obſervation and experiment, produces circumſpection and a quick ſenſe of difficulty. Beſides, I find that the ſyſtem-mongers, though a numerous body, have been but unſucceſsful ſince accurate obſervations are come into vogue. It is this circumſpection which prevents my proceeding inconſiderately from the ſcience of bodies to that of ideas. The ſea-anemonies give evident marks of ſenſibility; ſhall I thence conclude, that a ſoul animates them; or ſhall I grant that they are deprived of ſenſation, although favoured with the organs of it? I ſhall draw no concluſion, neither do I think myſelf at liberty to amplify upon that ſubject.

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ſition de ceux que j'ai eu lieu d'obſerver. Ce n'eſt cependant ni la crainte de paroître diffus, ni celle d'ennuier ceux pour leſquels j'écris qui m'a retenu; toute vérité leur eſt chére, toute découverte leur eſt précieuſe: mais l'habitude de contempler et d'expérimenter rend circonſpect, et fait appercevoir les difficultés: d'ailleurs les faiſeurs de ſyſtêmes, quoiqu'en grand nombre, ne ſont pas heureux depuis qu'on obſerve bien. Cette même circonſpection ne m'a pas permis de paſſer inconſidérément de la ſcience des corps dans celle des idées. Les anémones de mer me paroiſſent douées de ſenſations, en conclurai-je qu'un eſprit les anime, ou quelles ſont privées du ſentiment dont elles ont les organes? Je n'en concluerai rien, et je crois même mon mémoire peu ſuſceptible d'amplifications dans cette partie.

My very earlieſt obſervations ſhewed me that the ſea-anemonies feel and prognoſticate within doors the different changes of temperature in the atmoſphere. I had not leiſure at that time to form tables of their various indications; but I have ſince done it. This fact, if applied to practice, might be of uſe in the formation of a ſea-barometer, an object of no ſmall importance, which ſeveral ingenious men have hitherto endeavoured in vain to furniſh us with. I ſhould prefer the anemonies of the third ſpecies for this purpoſe, their ſenſation being very quick; they are alſo eaſily procured, and may be kept without nouriſhment. Five of them may be put in a glaſs veſſel, four inches wide and as many in depth, in which they will ſoon cleave to the angle formed by the ſides and the bottom. The water muſt be renewed every day, and, as they do not require a great quantity of it, as much may be fetched from the ſea (if they be kept on land) as will ſupply them for ſeveral days; its ſettling ſome

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Les anémones de mer reſſentent d'avance, et annoncent dans le cabinet les effets de la température; c'eſt ce que j'ai eu lieu de remarquer dès mes premieres obſervations; j'étois trop occupé alors pour en dreſſer des tables: je l'ai fait depuis. Ce fait mis en pratique pourra ſuppléer le baromêtre marin, dont pluſieurs ſavans ont fait inutilement la recherche juſqu' ici: ce ne ſeroit pas un petit avantage. Les anémones de la troiſieme eſpece m'ont paru plus propres que les autres à cet effet, parcequ'elles ſont très ſenſibles. Il eſt aiſé de ſ'en procurer, de les loger, de les conſerver ſans les nourrir &c. On mettra cinq de ces anémones dans un vaſe de verre d'environ quatre pouces de diamètre ſur autant de hauteur, elles ſ'y attacheront à l'angle que forme le fond avec les parois. Il faut renouveler l'eau tous les jours. Comme la quantité d'eau néceſſaire n'eſt pas grande, ſi on eſt à terre, on peut en faire prendre à la mer, pour pluſieurs jours; elle n'en ſera

ſome time will only improve it. I commonly renew the water every evening, becauſe if that change makes any impreſſion on the animals, it will not interfere with the principal obſervation, which, when I was forming my tables, I always took at a time equally diſtant from the renewals of the water. If the anemonies be at that time ſhut and contracted (as in fig. 14. of my firſt eſſay) I have reaſon to apprehend an approaching ſtorm; that is, high winds and a rough agitated ſea. When they are all ſhut, but not remarkably contracted, they forebode a weather ſomewhat leſs boiſterous, but ſtill attended with gales and a rough ſea. If they appear in the leaſt open, or alternately and frequently opening and cloſing, they indicate a mean ſtate both of winds and waves. When they are quite open, I expect tolerable fine weather and a ſmooth ſea. And laſtly, when their bodies are conſiderably extended and their limbs divergent, they ſurely prognoſticate fixed fair weather and a very calm

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ſera que meilleure étant reposée. Je ſuis dans l'uſage de renouveller l'eau le ſoir, parceque ſi ce changement d'eau fait quelque impreſſion aux anémones, il eſt alors compté pour rien; et quant à l'obſervation la plus notable, lorſque je dreſſois mes tables, je la faiſois à la moitié du jour. Si les anémones ſont toutes fermées, et retirées ſur elles mêmes, comme dans la figure 14 de mon premier mémoire, j'ai lieu de craindre qu'il ne ſurvienne quelque tempête, c'eſt à dire gros vent, la mer fort agitée, et troublée. Lorſqu'elles ſont toutes fermées ſans être retirées, cela annonce un temps un peu moins facheux, comme grand frais de vent, la mer agitée, &c. Si je vois mes anémones entrouvertes ou s'ouvrant et ſe fermant de moment à autre, c'eſt pour la mer et pour le vent un état moyen. Quand elles ſont ouvertes, j'attends un temps aſſez beau, la mer ſera peu agitée; enfin ont-elles le corps allongé, et les membres très étendus, c'eſt le préſage d'un beau fixe et d'une

calm ſea. There are times when ſome of the anemonies are open and others ſhut; the number muſt then be conſulted; the queſtion is decided by the majority. Accuracy in theſe obſervations is in a great meaſure acquired by habit. At ſea the glaſs may be ſwung, in the ſame manner as the compaſs, by ſuſpending it between two concentric circles, moving upon pivots that croſs each other at right angles, ſo that the rolling of the ſhip may agitate the water as little as poſſible. The anemonies that are uſed as *barometers* ſhould not be fed, for then the quantity of nouriſhment might influence their predictions. Anemonies of this and of the firſt ſpecies live and do well for ſeveral years, without taking any other food but what they find diſſeminated in the ſea water: but ſhould a reſpite of ſome days be granted them, they might then be fed with ſome pieces of muſcles or ſoft fiſh, and thus reſtored to their original vigour. Whenever

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d'une mer très calme. Il y a des momens où des anémones ſont ouvertes et d'autres fermées; alors on conclut par le plus grand nombre, on va pour ainſi dire à la pluralité des voix: l'habitude aide à ſe décider. En mer on poura ſuſpendre le verre, comme une bouſſole ou compas de mer, entre deux cercles concentriques ou balanciers, avec des pivots qui ſe croiſent à angles droits, afin que le tangage et les roulis du vaiſſeau n'agitent l'eau que le moins qu'il ſera poſſible. Il ne faut point nourrir les anémones qui ſervent de baromètre: car alors elles pourroient ſe fermer ou ſe tenir ouvertes, à raiſon de la nourriture qu'elles auroient priſe. Une anémone de cette eſpece, et de la premiere, vit très bien et en aſſez bon état, pendant pluſieurs années, ſans prendre d'autre nourriture que celle qu'elle trouve diſſeminée dans l'eau de la mer; cependant, ſi on avoit lieu de leur accorder une vacance de quelques jours, on pourroit les nourrir, et conſéquemment leur redonner leur premiére vigueur par quelques morceaux de moule ou de poiſſon mou. Lorſque le

ever the veſſel is ſullied by the ſediments of ſalts, ſlime, the firſt ſhoots of ſea-plants, &c. it may, on changing the water, be cleanſed by wiping it with a ſoft hair-pencil, or even with the finger, carefully avoiding, however, to rub or preſs hard upon the anemonies. Should any of them drop off during this operation, they may be left at liberty, for they will ſoon, of their own accord, fix themſelves to ſome other place. Should any of them die, which will ſoon be diſcovered by the milky colour of the water, and an offenſive ſmell on changing it, it muſt be taken out, and on the firſt opportunity, another of the ſame ſpecies be put in its place; thoſe of a moderate ſize are the moſt eligible. Such barometers might be applied as ornaments; for as they can be made by the perſons who are to uſe them, they might conſult their own taſte in the choice of the colours and ſhades of the limbs. A few anemonies of the firſt ſpecies might alſo be added, for as thoſe of the third generally take up their habitation

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le vaſe ſera ſali par le dépôt des ſels, le limon, ou par des commencemens de plantes marines, &c. on pourra en changeant l'eau, le nettoyer avec un pinceau doux, ou même avec le doit, obſervant de ne paſſer que légerement le doit ſur les anémones pour ne les pas comprimer. Si une anémone ſe détache, on la laiſſera en liberté, elle s'attachera en un autre lieu. Si elle vient à périr, ce qu'on reconnoitra par une très mauvaiſe odeur en changeant l'eau, et parceque cette eau devient un peu laiteuſe, on retirera l'anémone, et on lui en ſubſtituera à la premiere occaſion une autre toujours de même eſpece et de moyenne groſſeur; ce ſont les meilleures. Ce baromètre eſt agréable à la vue, car on peut choiſir des anémones dont les membres ſoient d'une couleur haute, douce, ou variée ſelon ſon gout, puis qu'on le conſtruit ſoi même: on peut encore y ajouter pluſieurs anémones de la premiere eſpece. Comme celles de la troiſieme ſe tiennent vers le fond du

tion at the bottom of the veſſel, theſe commonly keep near the ſurface of the water; and their various bay, crimſon, dark, purple, green, and other colours, would add no ſmall luſtre to the variegated object: nor ſhould their prognoſtications concerning the temperature of the air be altogether neglected. Perhaps they may at firſt produce young ones, which, if attended to, will be ſeen to encreaſe gradually. Should the anemonies of this firſt ſpecies not be uſed for obſervations, they might be fed and thus kept in a moſt flouriſhing condition. The form, ſize, and ornaments of this *barometer* may alſo be accommodated to any place or ſituation. The only inconvenience attending it would be the changing of the water every day; but even this amounts to no more than winding up our watches: neither is it abſolutely neceſſary to do it exactly at the ſame hour; nor is there any harm in the common practice of leaving the ſame water for ſeveral days, unleſs, indeed, it be inclined to

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du vaſe, celles-ci aiment à être à la ſurface de l'eau, leur couleur brun-rouge, cramoiſi, pourpre, foncé, vert, &c. fera un bel effet, et leur avis ſur la temperature n'eſt pas tout-à-fait à negliger. Elles pourront dans le commencement donner des petits, qu'on verra groſſir par la ſuite. Si on ne vouloit pas tenir compte des indications des anémones de cette premiere eſpece, en les nouriſſant elles ne ſeroient que plus belles. Ce baromètre eſt encore fort commode, aiſé à placer, et ſuſceptible de toute eſpece de forme et de décoration. Il peut être auſſi grand et auſſi petit qu'on veut. La ſeule incommodité eſt de changer l'eau chaque jour, ce qui n'eſt guère plus difficile que de remonter ſa montre, encore n'eſt-on pas ſtrictement obligé de le faire à la même heure, et il n'y a aucun inconvenient dans l'uſage ordinaire de laiſſer la même eau pendant pluſieurs jours, à moins qu'elle ne ſoit expoſée à ſe corrompre par une chaleur extrême, ou par quelqu'autre inconvénient peu

to corruption, by being expoſed to a great degree of heat, or by ſome other uncommon accident. If ever the *barometer* was to be removed, the veſſel might be left empty for a day or two, without any conſiderable detriment to the anemonies; they will contract, and continue fixed to the ſides, and reſume their wonted manœuvres, as ſoon as freſh ſea water is poured upon them. One neceſſary caution is, to diſpoſe of the barometer in a place where, as nearly as poſſible, the ſame degree of light prevails throughout the day: for it muſt be obſerved, that theſe animals, on being ſuddenly removed out of the dark into a ſtrong light will frequently contract in an inſtant, though they will ſoon recover after their uſual form. As the ſimple barometer, the Torricellian tube, which is the beſt of all, doth not always agree with the viſible ſtate of the local atmoſphere, and that frequently its indications are far from being exact; it cannot be expected that the ſea-anemonies ſhould excel in this reſpect, and that their indications

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peu ordinaire. On peut même vuider l'eau pour tranſporter le baromètre ou l'on veut, pendant un jour entier et plus, ſans que les anémones périſſent; elles ſe retireront ſur elles mêmes, et ſe tiendront attachées. On leur procurera de nouvelle eau de mer, et elles reprendront leur maneuvre ordinaire. Une attention néceſſaire c'eſt de placer autant qu'on le peut le baromètre d'anémones dans un lieu où il regne pendant tout le jour une lumiere à peu près égale: car on doit ſe ſouvenir, que ſouvent les anémones qui paſſent ſubitement d'un lieu obſcur à une grande lumiere ſe ferment; mais elles reprennent bientôt l'etat où elles auroient reſté. Comme le baromètre ſimple, le meilleur qui exiſte, le tube de Toricelli, n'eſt pas toujours parfaitment d'accord avec l'état viſible de l'atmoſphere du lieu où l'on obſerve, et qu'aſſez ſouvent ſes indications ne ſont pas d'une juſteſſe rigoureuſe, on ne doit pas s'attendre que les anémones de mer rempliſſent

indications ſhould always coincide with the above inſtrument. But as, with all its imperfections, the barometer abovementioned is of great utility, why ſhould not alſo this, formed of ſea-anemonies, be adopted notwithſtanding their little irregularities. Theſe are not the only animals that are influenced by the approaching changes of weather; the petrel, different ſorts of gulls, the ſcink, the leech, the little fiſh of the Danube, the cetaceous animals, the ſea-urchins, &c. are all ſaid to feel d prognoſticate thoſe variations.

Amidſt the more important objects of utility and neeſſity, every pleaſurable improvement ſhould not be totally neglected. I ſhall not therefore ſcruple to communicate what ideas have occurred to me, tending to this purpoſe. It appears to me, that the large anemonies, and even the ſmaller ones, being arranged in an advantageous manner, and properly grouped with acorns, barnacles, ſea-weeds, &c. might furniſh architecture, painting,

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pliſſent mieux cet objet, ni qu'elles ſoient toujours d'accord avec lui; et comme avec ſes inexactitudes, le baromètre eſt un bon inſtrument, on pourra ſe ſervir de celui d'anémones, independemment de ſes petits caprices. Ces animaux ne ſont pas les ſeuls qui reſſentent l'influence de la température prochaine, l'oiſeau-tempête, le petrel, les poulets de mer, le mabouja, la ſangſue, le petit poiſſon du Danube, les cétacées, les ourſins, &c. paſſent auſſi pour la reſſentir, et même pour l'annoncer.

Les vues d'utilité, de néceſſité, ne doivent point nous faire négliger celles d'agrément: je ne craindrai donc pas de dévoiler ici celles que j'ai ſur ce point. Il me ſemble que les grandes et même les petites anémones de mer priſes dans des poſitions variées les plus avantageuſes, et artiſtement groupées entr'elles ou avec les glands de mer, les conques anatifères, les plantes marines, &c; fourniroient

ing, ſculpture, and all the arts dependant upon drawing, with a variety of elegant and pleaſing ornaments, eſpecially in naval and hydraulic architecture, in which, contrary to all the maxims of true taſte, and regardleſs of probability, chimerical ornaments are often introduced, which have no manner of relation or affinity with the ſtructures they are intended to decorate. They are often ſeen affixed to ſubſtances to which the things they repreſent have little or no tendency to adhere; whereas the ſea-anemonies ſpontaneouſly cling to every ſubſtance in and even out of water, and might therefore being grouped into pictureſque feſtoons, be with great propriety ſubſtituted to the flowery garlands and other ſuch ornaments which prepoſterouſly decorate the ſterns of ſhips and the frizes on barges, and not ſeldom the moſt ſtately edifices, ſuch as bridges, ſluices, &c.

Expla-

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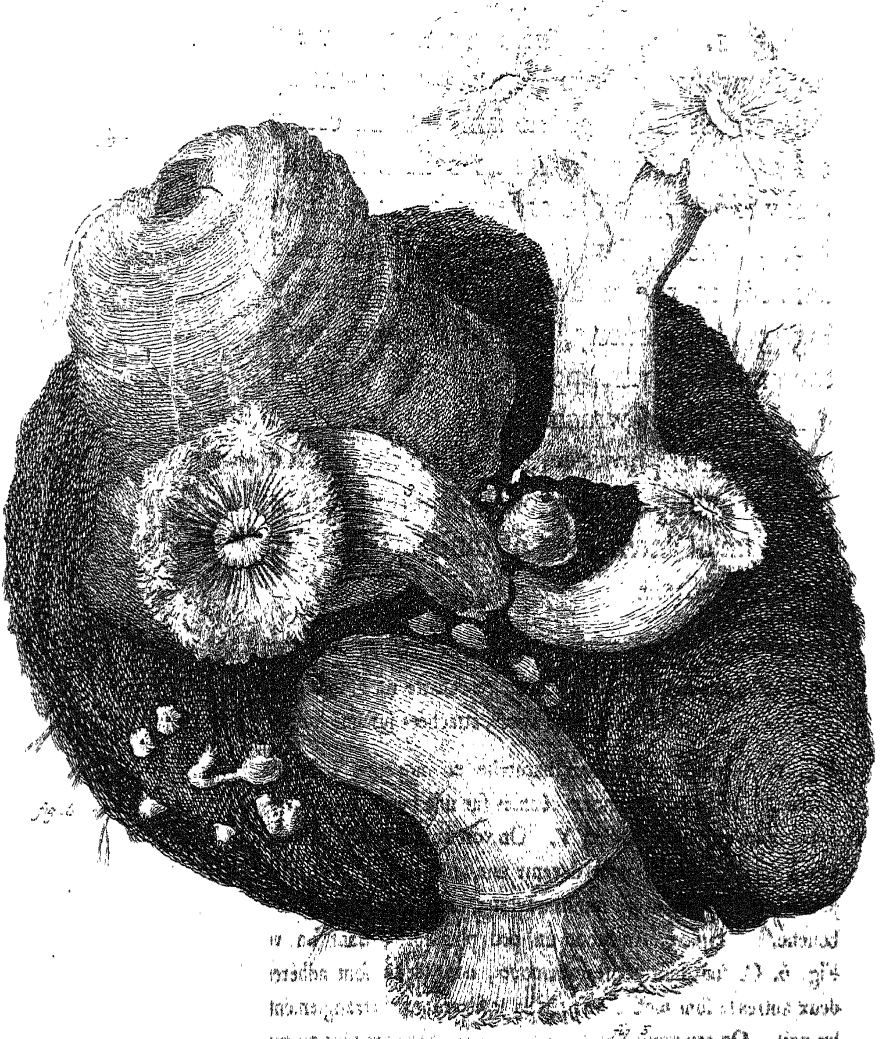
roient à l'architecture, à la peinture, à la ſculpture, et à tous les arts qui ont le deſſein pour baſe, de beaux ornemens, ſur tout pour l'architecture navale, et même pour l'architecture hydraulique, où, contre les règles du goût, contre la vraiſemblance, on employe ſouvent des ornemens chimériques et peu analogues au ſujet. On groupe, on attache ces objets à des lieux peu propres, peu diſposés à les recevoir; au contraire les anémones de mer s'attachent à tout ce qui ſe trouve dans l'eau et même hors de l'eau. Elles pourroient être ſubſtituées aux rozettes, former des groupes, des eſpeces de guirlandes pittoreſques dans la décoration des poupes des vaiſſeaux, dans les friſes qui ornent les canôts, et dans quelques édifices hydrauliques, comme les ponts, les écluſes, &c.

Explanation of TAB. VI. which repreſents a group of ſea-anemonies of the fourth ſpecies, adhering to an oyſter-ſhell.

Fig. 1. A contracted anemony of the natural and middling ſize adhering to the oyſter-ſhell. Fig. 2. Anemonies united to the ſame trunk, compared in the eſſay to the letter Y. At its baſis a little ſhred appears ready to be torn off in order to become a new anemony. Fig. 3. and 4. Two young anemonies moderately expanded, in the middle of which the mouths appear. Fig. 5. An anemony, ſomewhat more grown, on which the projecting rim appears. Fig. 6. Eight ſmall anemonies, two of which adhere together, as do alſo two others, which, however, are upon the point of being ſeparated by the contraction of the part that unites them. Other ſmall anemonies, of different ſizes, are ſeen upon the oyſter-ſhell.

Explication des Figures. La planche repreſente un groupe d'anémones de mer de la quatrieme eſpece, attachées ſur une huitre.

Fig. 1. Anémone de groſſeur naturelle et moyenne attachée ſur l'huitre et fermée. Fig. 2. Anémones réunies ſur une meme tige, elle eſt déſignée dans le memoire par la forme d'un Y. On voit au bord de ſa baſe un petit lambeau pret à en être détaché pour devenir une anémone. Fig. 3. et 4. ſont deux jeunes anémones plus ou moins ouvertes, au milieu deſquelles on remarque la bouche. Fig. 5. Anémone un peu plus forte, dont on voit le bourlet. Fig. 6. Ce ſont huit petites anémones, dont deux ſont adhérentes enſemble; deux autres le ſont auſſi, mais prêtes à ſe ſéparer par l'étranglement de la partie qui les unit. On remarque ſur l'huitre d'autres anémones plus ou moins petites.



DICQUEMARE. DEL. J. Beſire. Sculp.

XXI. *Account of the Sea-Cow, and the Uſe made of it.*

By Molineux Shuldham, *Eſquire.*

Redde, Mar. 2, 1775.

THE ſea-cow is a native of the Magdalen Iſlands, St. John's, and Anticoſti in the Gulph of St. Lawrence. They reſort very early in the ſpring to the former of theſe places, which ſeems to be by nature particularly adapted to the wants of theſe animals, abounding with (a) clams of a very large ſize, and the moſt convenient landing places, called Echouries. Here they crawl up in great numbers, and ſometimes remain for fourteen days together without food, when the weather is fair; but on the firſt appearance of rain, they immediately retreat to the water with great precipitation. They are, when out of the water, very unweildy, and move with great difficulty. They weigh from 1500 to 2000 pounds, producing, according to their ſize, from one to two barrels of oil, which is boiled out of a fat ſubſtance that lies between the ſkin and the fleſh. Immediately on their arrival they calf, and engender again about two months after; ſo that they carry their young about nine months. They never have more than two at a time, and ſeldom more than one.

(a) A ſhell-fiſh reſembling a ſcallop.

The echouries are formed principally by nature, being a gradual ſlope of ſoft rock, with which the Magdalen iſlands abound, about 80 or 100 yards wide at the water ſide, and ſpreading ſo as to contain, near the ſummit, a very conſiderable number. Here they are ſuffered to come on ſhore and amuſe themſelves for a conſiderable time, till they acquire a boldneſs, being at their firſt landing ſo exceedingly timid as to make it impoſſible for any perſon to approach them. In a few weeks they aſſemble in great numbers; formerly, when undiſturbed by the Americans, to the amount of ſeven or eight thouſand; and the form of the echourie not allowing them to remain contiguous to the water, the foremoſt ones are inſenſibly puſhed above the ſlope. When they are arrived to a convenient diſtance the fiſhermen, having provided the neceſſary apparatus, take the advantage of a ſea wind, or a breeze blowing rather obliquely on the ſhore, to prevent the ſmelling of theſe animals (who have that ſenſe in great perfection, contributing to their ſafety), and with the aſſiſtance of very good dogs, endeavour in the night time to ſeparate thoſe that are the fartheſt advanced from thoſe next the water, driving them different ways. This they call making a cut, and is generally looked upon to be a moſt dangerous proceſs, it being impoſſible to drive them in any particular direction, and difficult to avoid them; but as they are advanced above the ſlope of the echourie, the darkneſs of the night deprives them of every direction to the water, ſo that they ſtray about and are killed at leiſure, thoſe that are neareſt the ſhore being the firſt victims. In this manner there has been killed fifteen or ſixteen hundred at one cut. They then ſkin them, and take off a coat of fat that always ſurrounds them, which they diſſolve by heat into oil. The ſkin is cut into ſlices of two or three inches wide, and exported to America for carriage traces, and to England for glue. The teeth is an inferior ſort of ivory, and is manufactured for the ſame purpoſes, but very ſoon turns yellow.

XXII. *The Proceſs of making Ice in the* Eaſt Indies. *By Sir* Robert Barker, *F. R. S. in a Letter to Dr.* Brockleſby.

TO DR. RICHARD BROCKLESBY, F. R. S.

SIR,

Spring-Gardens,
March 2, 1775.

Redde, Mar. 9, 1775.

THE proceſs of making ice in the Eaſt Indies having become a ſubject of ſpeculation, I beg permiſſion to preſent you with the method by which it was performed at Allahabad, Mootegil, and Calcutta, in the Eaſt Indies, lying between 25½ and 23½ degrees of North latitude. At the latter place I have never heard of any perſons having diſcovered natural ice in the pools or ciſterns, or in any waters collected in the roads; nor has the thermometer been remarked to deſcend to the freezing point; and at the former very few only have diſcovered ice, and that but ſeldom. But in the proceſs of making ice at theſe places it was uſual to collect a quantity every morning, before Sun-riſe (except in ſome particular kinds of weather, which I ſhall ſpecify in the ſequel), for near three months in the year; *viz.* from December till February.

The ice-maker belonging to me at Allahabad (at which place I principally attended to this enquiry) made a ſufa ſufficient quantity in the winter for the ſupply of the table during the ſummer ſeaſon. The methods he purſued were as follows: on a large open plain, three or four excavations were made, each about thirty feet ſquare and two deep; the bottoms of which were ſtrewed about eight inches or a foot thick with ſugar-cane, or the ſtems of the large Indian corn dried. Upon this bed were placed in rows, near to each other, a number of ſmall, ſhallow, earthen pans, for containing the water intended to be frozen. Theſe are unglazed, ſcarce a quarter of an inch thick, about an inch and a quarter in depth, and made of an earth ſo porous, that it was viſible, from the exterior part of the pans, the water had penetrated the whole ſubſtance. Towards the duſk of the evening, they were filled with ſoft water, which had been boiled, and then left in the afore-related ſituation. The ice-makers attended the pits uſually before the Sun was above the horizon, and collected in baſkets what was frozen, by pouring the whole contents of the pans into them, and thereby retaining the ice, which was daily conveyed to the grand receptacle or place of preſervation, prepared generally on ſome high dry ſituation, by ſinking a pit of fourteen or fifteen feet deep, lined firſt with ſtraw, and then with a coarſe kind of blanketing, where it is beat down with rammers, till at length its own accumulated cold again freezes and forms one ſolid maſs. The mouth of the pit is well ſecured from the exterior air with ſtraw and blankets, in the manner of the lining, and a thatched roof is thrown over the whole. It is here neceſſary to

From theſe circumſtances it appears, that water, by being placed in a ſituation free from receiving heat from other bodies, and expoſed in large ſurfaces to the air, may be brought to freeze when the temperature of the atmoſphere is ſome degrees above the freezing point on the ſcale of FAHRENHEIT's thermometer; and by being collected and amaſſed into a large body, is thus preſerved, and rendered fit for freezing other fluids, during the ſevere heats of the ſummer ſeaſon. In effecting which there is alſo an eſtabliſhed mode of proceeding; the ſherbets, creams, or whatever other fluids are intended to be frozen, are confined in thin ſilver cups of a conical form, containing about a pint, with their covers well luted on with paſte, and placed in a large veſſel filled with ice, ſaltpetre, and common ſalt, of the two laſt an equal quantity, and a little water to diſſolve the ice and combine the whole. This compoſition preſently freezes the contents of the cups to the ſame conſiſtency of our ice creams, &c. in Europe; but plain water will become ſo hard as to require a mallet and knife to break it. Upon applying the bulb of a thermometer to one of theſe pieces of ice, thus frozen, the quickſilver has been known to ſink two or three degree below the freezing point: ſo that from an atmoſphere apparently not mild enough to produce natural ice, ice ſhall be formed, collected, and a cold accumulated, that ſhall cauſe the quickſilver to fall even below the freezing point. The promiſing advantages of ſuch a diſcovery could alone induce the Aſiatic (whoſe principal ſtudy is the luxuries of life, and this may well be called ſuch, when I have often regaled with ices when the thermometer has ſtood at 112°), to make an attempt of profiting by ſo very ſhort a duration of cold during the nights in theſe months, and by a well-timed and critical contrivance of ſecuring this momentary degree of cold, they have procured to themſelves a comfortable refreſhment as a recompence, to alleviate, in ſome degree, the intenſe heats of the ſummer ſeaſon, which, in ſome parts of India, would be ſcarce ſupportable, but by the aſſiſtance of this and many other inventions.

Accompanying I beg leave to offer you ſome obſervations, I made in the year 1767 (b), in the province of Allahabad, on the temperature of the weather, which will ſerve to elucidate the extraordinary and ſudden changes incident to that part of Aſia. Alſo ſome remarks on the weather during my voyage to England, particularly ſpecifying the ſituation we were in when the obſervations were made.

I am, SIR, with regard,

Your moſt obedient humble ſervant,

ROBERT BARKER.

(b) See a general account of theſe obſervations Article XVIII.

XXIII. *Of the* Houſe-Swallow, Swift, *and* Sand-Martin*. *By the Rev.* Gilbert White, *in Three Letters to the Hon.* Daines Barrington, *F. R. S.*

LETTER I.

OF THE HOUSE-SWALLOW.

DEAR SIR, Selborne, Jan. 29, 1775.

Redde, Mar. 16, 1775.

THE houſe-ſwallow, or chimney-ſwallow, is undoubtedly the firſt comer of all the Britiſh *hirundines*; and appears in general on or about the 13th of April, as I have remarked from many years obſervation. Not but now and then a ſtraggler is ſeen much earlier; and in particular, when I was a boy, I obſerved a ſwallow, for a whole day together, on a ſunny warm Shrove-tueſday, which day could not fall out later than the middle of March, and often happens early in February. It is worth remarking, that theſe birds are ſeen firſt about lakes and mill-ponds: and it is alſo very particular, that if theſe early viſitors happen to find froſt and ſnow, as was the caſe in the two dreadful ſprings of 1770 and 1771, they immediately withdraw for a time. A circumſtance this much more in favour

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* See an account of the Houſe-Martin by the ſame gentleman, in Phil. Tranſ. vol. LXIV. p. 196.

of

hiding than migration; ſince it is much more probable, that a bird ſhould retire to its *hybernaculum* juſt at hand, than return for a week or two only to warmer latitudes. This ſwallow, though called the chimney-ſwallow, by no means builds altogether in chimneys; but often within barns and out-houſes, againſt the rafters: and ſo ſhe did in Virgil's time:

> —— "*Antè*
> "*Garrula quàm tignis nidos ſuſpendat hirundo.*"

In Sweden ſhe builds in barns, and is called *ladu ſwala*, the barn-ſwallow. Beſides, in the warmer parts of Europe, there are no chimneys to houſes except they are Engliſh-built. In theſe countries ſhe conſtructs her neſt in porches and gateways, galleries, and open halls. Here and there a bird may affect ſome odd, peculiar place; as we have known a ſwallow build down the ſhaft of an old well, through which chalk was formerly drawn up, for the purpoſes of manure; but in general with us, this *hirundo* breeds in chimneys, and loves to haunt thoſe ſtacks where there is a conſtant fire, no doubt for the ſake of warmth. Not that it can ſubſiſt in the immediate ſhaft where there is a fire; but prefers one adjoining to that of the kitchen, and diſregards the perpetual ſmoke of that funnel, as I have often obſerved with ſome degree of wonder. Five or ſix or more feet down the chimney does this little bird begin to form her neſt about the middle of May, which conſiſts, like that of the houſe-martin, of a cruſt or ſhell, compoſed of dirt and mud, mixed with ſhort pieces of ſtraw, to render it tough and permanent; with this difference, that whereas the ſhell of the martin is nearly hemiſpheric, that of the ſwallow is open at the top, and like half a deep diſh. This neſt is lined with fine graſſes and feathers, which are often collected as they float in the air. Wonderful is the addreſs which this adroit bird ſhews all day long in aſcending and deſcending, with ſecurity, through ſo narrow a paſs. When hovering over the mouth of the funnel, the vibrations of her wings, acting on the confined air, occaſion a rumbling like thunder. It is not improbable, that the dam ſubmits to this inconvenient ſituation ſo low in the ſhaft, in order to ſecure her broods from rapacious birds, and particularly from owls, which frequently fall down chimnies, perhaps in attempting to get at theſe neſtlings. The ſwallow lays from four to ſix white eggs, dotted with red ſpecks; and brings out her firſt brood about the laſt week in June, or the firſt week in July. The progreſſive method by which the young are introduced into life is very amuſing. Firſt they emerge from the ſhaft with difficulty enough, and often fall down into the rooms below. For a day or ſo they are fed on the chimney-top; and then are conducted to the dead leafleſs bough of ſome tree, where, ſitting in a row, they are attended with great aſſiduity, and may then be called *perchers*. In a day or two more they become *flyers*, but are ſtill unable to take their own food; therefore they play about near the place where the dams are hawking for flies; and when a mouthful is collected, at a certain ſignal given, the dam and the neſtling advance riſing towards each other, and meeting at an angle; and the young one all the while uttering ſuch a little quick note of gratitude and complacency, that a perſon muſt have paid very little regard to the wonders of nature that has not often remarked this feat. The dam betakes herſelf immediately to the buſineſs of a ſecond brood, as ſoon as ſhe is diſengaged from her firſt; which at once aſſociates with the firſt broods of houſe-martins, and with them congregates, cluſtering on ſunny roofs, towers, and trees. This *hirundo* brings out her ſecond brood towards the middle and end of Auguſt. All the ſummer long is the ſwallow a moſt inſtructive pattern of unwearied induſtry and affection! For, from morning to night, while there is a family to be ſupported, ſhe ſpends the whole day in ſkimming cloſe to the ground, and exerting the moſt ſudden turns and quick evolutions. Avenues and long walks under hedges, paſture fields and mown meadows where cattle graze, are her delight, eſpecially if there are trees interſperſed; becauſe in ſuch ſpots inſects moſt abound. When a fly is taken a ſmart ſnap from her bill is heard, reſembling the noiſe at the ſhutting of a watch-caſe; but the motion of the mandibles is too quick for the eye. The ſwallow, probably the male bird, is the *excubitor* to the houſe-martins, and other little birds, announcing the approach of birds of prey: for as ſoon as an hawk appears, with a ſhrill alarming note, he calls all the ſwallows and martins about him, who purſue in a body, whilſt they buffet and ſtrike their enemy till they have driven him from the village, dart-every circumſtance that does not immediately reſpect ſelf-preſervation, or lead at once to the propagation or ſupport of their ſpecies. I am, with all reſpect, &c.

LETTER II.

Of the SWIFT, or BLACK MARTIN.

DEAR SIR, Selborne, Sept. 28, 1774.

AS the *Swift* or *Black Martin* is the largeſt of the Britiſh *hirundines*, ſo it is undoubtedly the lateſt comer: for I remember but one inſtance of its appearing before the laſt week in April; and in ſome of our late froſty, harſh ſprings it has not been ſeen till the beginning of May. This ſpecies uſually arrives in pairs. The ſwift, like the ſand-martin, is very defective in architecture, making no cruſt or ſhell for its neſt; but forming it of dry graſſes and feathers, very rudely and inartificially put together. With all my attention to theſe birds I have never been able to diſcover one in the act of collecting or carrying in materials: ſo that I have ſuſpected (ſince their neſts are exactly the ſame) that they ſometimes uſurp upon the houſe-ſparrows and expel them, as ſparrows do the houſe and ſand-martin; well remembering that I have ſeen them ſquabbling together at the entrance of their holes, and the ſparrows up in arms and much diſconcerted at theſe intruders. And yet I am aſſured, by a nice obſerver in ſuch matters, that they do collect feathers for their neſts in Andaluſia; and that he has ſhot them with ſuch materials in their mouths. Swifts, like ſand-martins, carry on the buſineſs of nidification quite in the dark, in crannies of towers and ſteeples, and upon the tops of the walls of churches under the roof; and therefore cannot be ſo narrowly watched as thoſe ſpecies that build more openly; but from what I could ever obſerve, they begin neſting about the middle of May, and I have remarked, from eggs taken, that they have ſat hard by the ninth of June. In general they haunt high buildings, churches, and ſteeples, and build only in ſuch; yet in this village ſome pairs frequent the loweſt and meaneſt cottages, and educate their young under thoſe thatched roofs. We remember but one inſtance where they bred out of buildings; and that is in the ſides of a deep chalk-pit near the town of Odiham in this county*, where we have ſeen many pairs entering the crevices and ſkimming and ſqueaking round the precipices. As I have regarded theſe amuſive birds with great attention, if I ſhould advance ſomething new and peculiar with reſpect to them, and different from all other birds, I might perhaps be credited; eſpecially as my aſſertion is the reſult of many years exact obſervation. The fact that I would advance is, that ſwifts *tread* or copulate on the wing: and I could wiſh any nice obſerver, that is ſtartled at this ſuppoſition, to uſe his own eyes, and I think he will ſoon be convinced. In another claſs of animals, *videlicet*, the *inſect*, nothing is ſo common as to ſee the different ſpecies of many *genera* in conjunction as they fly. The ſwift is almoſt continually on the wing; and as it never ſettles on the ground, on trees, or roofs, would ſeldom find opportunity for amorous rites, was it not enabled to indulge them in the air. If any perſon would watch theſe birds on a fine morning in May, as they are ſailing round at a great height from the ground, he would ſee every now and then one drop on the back of another, and both of them ſink down together for many fathoms, with a loud piercing ſhriek. This I take to be the juncture when the buſineſs of generation is carrying on. As the ſwift eats, drinks, collects materials for its neſt, and, as it ſeems, propagates on the wing; it appears to live more in the air than any other bird, and to perform all functions there, ſave thoſe of ſleeping and incubation. This *hirundo* differs widely from its congeners, in laying invariably but *two eggs* at a time, which are milk-white, long, and peaked at the ſmall end; whereas the other ſpecies lay at each brood from *four* to *ſix*. It is a moſt alert bird, riſing very early, and retiring to reſt very late, and is on the wing, in the height of ſummer, at leaſt ſixteen hours. In the longeſt days it does not withdraw to reſt till a quarter before nine in the evening, being the lateſt of all day-birds. Juſt before they retire, whole groups of them aſſemble in the air, and ſqueak and ſhoot about with wonderful rapidity. But this bird is never ſo much alive as in ſultry, thundry weather, when it expreſſes great alacrity, and calls forth all its powers. In hot mornings, ſeveral getting together in little parties, daſh round the ſteeples and churches, ſqueaking as they go in a very clamorous manner: theſe, by nice obſervers, are ſuppoſed to be males ſerenading their ſitting hens; and not without reaſon, ſince they never ſqueak till they come cloſe to the walls or eaves; and ſince thoſe within utter at the ſame time a little inward note of complacency. When the hen has ſat hard all day, ſhe ruſhes forth juſt as it is almoſt dark, when ſhe ſtretches and relieves her weary limbs, and ſnatches a ſcanty meal for a few minutes, and then returns to her duty of incubation. Swifts when wantonly and cruelly ſhot, while they have young, diſcover a little lump of inſects in their mouths, which they pouch and hold under their tongue. In general they feed in a much higher diſtrict than the other ſpecies; a proof that gnats and other inſects do alſo abound to a conſiderable height in the air. They alſo range to great diſtance, ſince loco-motion is no labour to them, who are endowed with ſuch vaſt powers of wing. Their powers ſeem to be in proportion to their levers; and their wings are longer in proportion than thoſe of almoſt any other bird. When they mute, or eaſe themſelves, in flight, they raiſe their wings, and make them meet over their heads. At ſome certain times in the ſummer I had remarked, that ſwifts were hawking low for hours together, over pools and ſtreams; and could not help enquiring into the object of their purſuit, that induced them to deſcend ſo much below their uſual range. After ſome trouble, I found that they were taking *phryganeæ*, *ephemeræ*, and *libellulæ* (cadew-flies, may-flies, and dragon-flies), that were juſt emerged out of their aurelia-ſtate. I then no longer wondered that they ſhould be ſo willing to ſtoop for a prey, that yielded them ſuch plentiful and ſucculent nouriſhment. They bring out their young about the middle or latter end of July; but as theſe never become perchers, nor, that ever I could diſcern, are fed on the wing by their dams, the coming forth of the young is not ſo notorious as in the other ſpecies. On the 30th of June laſt, I untiled the eaves of a houſe where many pairs build, and found in each neſt only *two* naked, ſquab *pulli*. On the eighth of July I repeated the ſame enquiry, and found they had made very little progreſs towards a fledged ſtate; but were ſtill naked and helpleſs. From whence we may conclude, that birds, whoſe way of life keeps them perpetually on the wing, would not be able to quit their neſt till the end of the month. Swallows and martins, that have numerous families, are continually feeding them every two or three minutes; while ſwifts, that have but two young to maintain, are much at their leiſure, and do not attend on their neſts for hours together. Sometimes they purſue and ſtrike at hawks that come in their way; but not with that vehemence and fury that ſwallows expreſs on the ſame occaſion. They are out all day long in wet days, feeding about, and diſregarding ſtill rain: from whence two things may be gathered; firſt, that many inſects abide high in the air, even in rain; and next, that the feathers of theſe birds muſt be well preened to reſiſt ſo much wet. Windy, and particularly windy weather with heavy ſhowers, they diſlike; and on ſuch days withdraw, and ſcarce ever are ſeen. There is a circumſtance reſpecting the *colour* of ſwifts, which ſeems not to be unworthy our attention. When they appear in the ſpring they are all over of a gloſſy, dark, ſoot-colour, except their chins, which are white; but by being all day long in the ſun and air they become quite weather-beaten and bleached before they depart; and yet they return gloſſy again in the ſpring. Now if they purſue the ſun into lower latitudes, as ſome ſuppoſe, in order to enjoy a perpetual ſummer, why do they not return bleached? Do they not rather, perhaps, retire to reſt for a ſeaſon, and at that juncture moult and change their feathers, ſince all other birds are known to moult ſoon after the ſeaſon of breeding? Swifts are very anomalous in many particulars, diſſenting from all their congeners not only in the number of their young, but in breeding but *once* in a ſummer; whereas all the other Britiſh *hirundines* breed invariably *twice*. It is paſt all doubt, that ſwifts can breed but once, ſince they withdraw in a very ſhort time after the flight of their young, and ſome time before their congeners bring out their ſecond broods. We may here remark, that as ſwifts breed but *once* in a ſummer, and only *two* at a time, and the other *hirundines* twice, the latter, who lay from *four* to ſix eggs, increaſe at an average five times as faſt as the former. But in nothing are ſwifts more ſingular than in their early retreat. They retire, as to the main body of them, by the 10th of Auguſt, and ſometimes a few days ſooner; and every ſtraggler invariably withdraws by the 20th, while their congeners, all of them, ſtay till the beginning of October, many of them ſtay all through the month, and ſome occaſionally to the beginning of November. This early retreat is myſterious and wonderful, ſince that time is often the ſweeteſt ſeaſon in the year. But what is more wonderful, they retire ſtill earlier in the moſt ſoutherly parts of Andaluſia, where they can be no ways influenced by any defect of heat; or, as one might ſuppoſe, by any defect of food. Are they regulated in their motions with us by a failure of food? or by a propenſity to moulting? or by a diſpoſition to reſt after ſo rapid a life? or by what? This is one of thoſe incidents in natural hiſtory that not only baffles our ſearches, but almoſt eludes our gueſſes! Theſe *hirundines* never perch on trees or roofs, and ſo never congregate with their congeners. They are fearleſs while haunting their neſting places; are not to be ſcared with a gun; and are often beaten down with poles, as they ſtoop to go under the eaves. They are alſo much infeſted with thoſe peſts to the whole genus, called *hippoboſcæ hirundinis*; and often wriggle and ſcratch themſelves in their flight to get rid of theſe clinging annoyances. Swifts are no ſongſters, and have only one harſh ſcreaming note; yet there are ears to which that note is not diſpleaſing from an agreeable aſſociation of ideas, ſince that note never occurs but in the moſt lovely ſummer weather. They never ſettle on the ground but through accident, and when down can hardly riſe, on account of the ſhortneſs of their legs and the length of their wings: neither can they walk, but only crawl; but they have a ſtrong graſp with their feet, by which they cling to walls. Their bodies being flat, they can enter into a very narrow crevice; and where they cannot paſs on their bellies, they will turn up edgeways. The particular formation of the ſwift's foot diſcriminates that bird from all the Britiſh *hirundines*, and indeed from all other known birds, the *hirundo melba*, or great white-bellied ſwift of Gibraltar, excepted; for it is ſo diſpoſed as to carry *omnes quatuor digitos anticos*. Beſides, the leaſt toe, which ſhould be the back toe, conſiſts only of one bone alone; and the other three only of two apiece. A conſtruction moſt rare and peculiar; but nicely adapted to the purpoſes in which their feet are employed. This, and ſome peculiarities attending the noſtrils and under mandible, have induced a diſcerning naturaliſt(a) to ſuppoſe, that this ſpecies might conſtitute a *genus per ſe*. In London, a party of ſwifts frequent the Tower, playing and feeding over the river juſt below the bridge: others haunt ſome of the churches of the Borough next the fields; but do not venture, like the houſe-martin, into the cloſe, crowded parts of the town. The Swedes have beſtowed a very pertinent name on this ſwallow, calling it *ring ſwala* from the perpetual *rings* or circles that it takes round the ſcene of its nidification. I am, &c.

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* *Viz.* Hampſhire.

(a) JOHN ANTONY SCOPOLI, of Carniola, M. D.

LETTER III.

Of the SAND-MARTIN, or BANK-MARTIN.

DEAR SIR, Selborne, Feb. 26, 1774.

THE Sand-martin, or Bank-martin, is by much the leaſt of any of the Britiſh *hirundines*, and as far as ever we have ſeen, the ſmalleſt known *hirundo*; though BRISSON aſſerts that there is one much ſmaller, and that is the *hirundo eſculenta*. But it is much to be regretted, that it is ſcarce poſſible for any obſerver to be ſo full and exact as he could wiſh, in reciting the circumſtances attending the life and converſation of this little bird; ſince it is *feræ naturâ*, at leaſt in this part of the kingdom, diſclaiming all domeſtic attachments, and haunting wild heaths and commons where there are large lakes, while the other ſpecies, eſpecially the ſwallow and houſe-martin, are remarkably gentle and domeſticated, and never ſeem to think themſelves ſafe but under the protection of man. Here are in this pariſh, in the ſand-pits, and banks of the lakes of Woolmer Foreſt, ſeveral colonies of theſe birds; and yet they are never ſeen in the village, nor do they at all frequent the cottages that are ſcattered about in that wild diſtrict. The only inſtance, I ever remember, where this ſpecies haunts any building, is at the town of Biſhop's Waltham in this county, where many ſand-martins neſtle and breed in the ſcaffold-holes of the back-wall of WILLIAM OF WICKHAM's ſtables; but then this wall ſtands in a very ſequeſtered and retired encloſure, and faces upon a large and beautiful lake. And indeed this ſpecies ſeems ſo to delight in large waters, that no inſtance occurs of their abounding but near vaſt pools or rivers; and in particular, it has been remarked, that they ſwarm on the banks of the Thames, in ſome places below bridge. It is curious to obſerve with what different degrees of architectonic ſkill Providence has endowed birds of the ſame *genus*, and ſo nearly correſpondent in their general mode of life! For while the ſwallow and the houſe-martin diſcover the greateſt addreſs in raiſing and ſecurely fixing cruſts or ſhells of loam as *cunabula* for their young, the bank-martin terebrates a round and regular hole in the ſand or earth, which is ſerpentine, horizontal, and about two feet deep. At the inner end of this burrow does this bird depoſit, in a good degree of ſafety, her rude neſt, conſiſting of fine graſſes and feathers, uſually gooſe feathers, very inartificially laid together. Perſeverance will accompliſh any thing; though one would at firſt be diſinclined to believe, that this weak bird, with her ſoft and tender bill and claws, ſhould ever be able to bore the ſtubborn ſand-bank without entirely diſabling herſelf. Yet with theſe feeble inſtruments have I ſeen a pair of them make good diſpatch, and could remark how much they had ſcooped that day, by the freſh ſand which ran down the bank, and was of a different colour from that which lay looſe and bleached in the Sun. In what ſpace of time theſe little artiſts are able to mine and finiſh theſe cavities I have never been able to diſcover, for reaſons given above; but it would be a matter worthy of obſervation, where it falls in the way of any naturaliſt to make his remarks. This I have often taken notice of, that ſeveral holes, of different depths, are left unfiniſhed at the end of ſummer. To imagine that theſe beginnings were intentionally made in order to be in greater forwardneſs for next ſpring, is allowing, perhaps, too much foreſight and *rerum prudentia* to a ſimple bird. May not the cauſe of theſe *latebræ* being left unfiniſhed ariſe from their meeting in thoſe places with *ſtrata* too hard, harſh, and ſolid for their purpoſe, which they relinquiſh, and go to a freſh ſpot that works more freely? Or may they not in other places fall in with a ſoil as much too looſe and mouldering, liable to flounder, and threatening to overwhelm them and their labours?

One thing is remarkable, that after ſome years the old holes are forſaken, and new ones bored; perhaps becauſe the old habitations grow foul and fetid from long uſe; or becauſe they may ſo abound with fleas as to become untenantable. This ſpecies of ſwallow, moreover, is ſtrangely annoyed with fleas; and we have ſeen fleas, bed-fleas *(pulex irritans)* ſwarming at the mouths of theſe holes like bees on the ſtools of their hives. When they happen to breed near hedges and encloſures, they are diſpoſſeſſed of their breeding holes by the houſe-ſparrow, which is on the ſame account a fell adverſary to houſe-martins. The following circumſtance ſhould by no means be omitted, that theſe birds do *not* make uſe of theſe caverns by way of *hybernacula*, as might be expected; ſince banks ſo perforated have been dug out with care in the winter, when nothing was found but empty neſts. The ſand-martin arrives much about the ſame time with the ſwallow, and lays, as ſhe does, from four to ſix white eggs. But as this ſpecies is *cryptogame*, carrying on the buſineſs of nidification, incubation, and the ſupport of its young, in the dark, it would not be ſo eaſy to aſcertain the time of breeding, were it not for the coming forth of the broods, which appear much about the time, or rather ſomewhat earlier than thoſe of the ſwallow. The neſtlings are ſupported in common like thoſe of the *congeners*, with gnats and other ſmall inſects; and ſometimes they are fed with *libellulæ* (dragon-flies) almoſt as long as themſelves. The laſt week in June, we have ſeen a row of theſe ſitting on a rail near a great pool, as perchers, and ſo young and helpleſs, as eaſily to be taken by hand; but whether the dams ever feed them on the wing, as ſwallows and houſe-martins do, we have never yet been able to determine; nor do we know whether they purſue and attack birds of prey. Theſe *hirundines* are no ſongſters, but rather mute, making only a little harſh noiſe when a perſon approaches their neſts. They ſeem not to be of a ſociable turn, never with us congregating with their *congeners* in the autumn. Undoubtedly they breed a ſecond time, like the houſe-martin and ſwallow, and withdraw about Michaelmas. Though in ſome particular diſtricts they may happen to abound, yet in the whole, in the South of England at leaſt, is this much the rareſt ſpecies. For there are few towns or large villages but what abound with houſe-martins; few churches, towers, or ſteeples, but what are haunted by ſome ſwifts; ſcarce a hamlet or ſingle cottage-chimney that has not its ſwallow: while the bank-martins, ſcattered here and there, live a ſequeſtered life among ſome abrupt ſand-hills, and in the banks of ſome few rivers. Theſe birds have a peculiar manner of flying; flitting about with odd jerks and vacillations, not unlike the motions of a butterfly. Doubtleſs the flight of all *hirundines* is influenced by, and adapted to, the peculiar ſort of inſects which furniſh their food. Hence it would be worth enquiry to examine, what particular *genus* of inſects affords the principal food of each reſpective ſpecies of ſwallow. Sand-martins differ from their *congeners* in the diminutiveneſs of their ſize and in their colour, which is what is uſually called a mouſe-colour. Near Valentia in Spain they are taken and ſold in the markets for the table; and are called by the country people, probably from their deſultory, jerking manner of flight, *papilion de Montagna*.

I am, with the greateſt reſpect, &c.

Fig. A

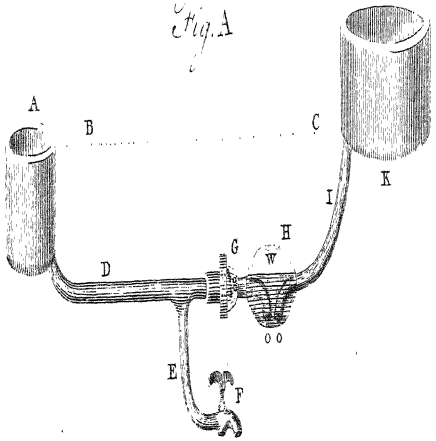


Fig. B.

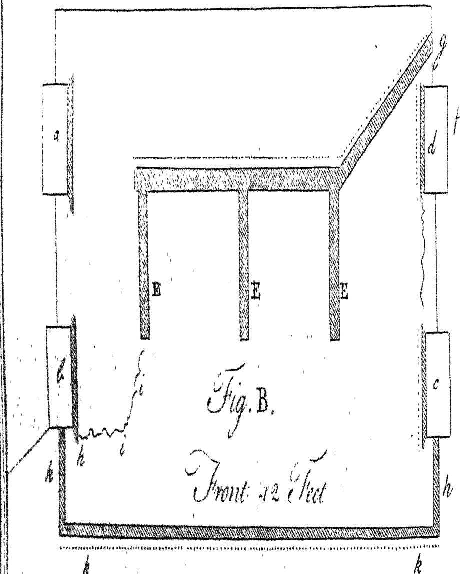


Fig. 1.

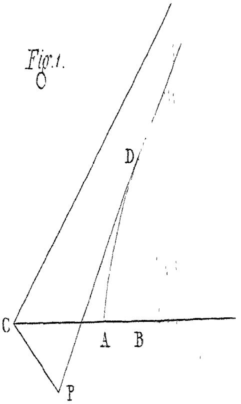


Fig. 2.

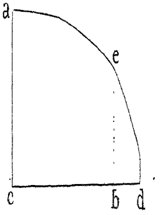


Fig. 3.

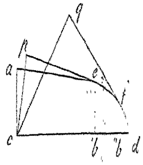
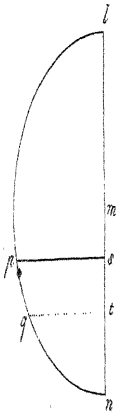


Fig. 4.



Basire sc.

XXIV. *Account of a Machine for raiſing Water, executed at* Oulton, *in* Cheſhire, *in* 1772. *In a Letter from Mr.* John Whitehurſt *to Dr.* Franklin.

DEAR SIR,

Redde, Mar. 16, 1775.

PRESUMING the mode of raiſing water by its *momentum* may be new and uſeful to many individuals, induces me to ſend you the incloſed plan and deſcription of a work, executed in the year 1772, at Oulton, in Cheſhire, the ſeat of PHILIP EGERTON, Eſq. for the ſervice of a brewhouſe and other offices, and is found to anſwer effectually. I am, SIR,

Your moſt obedient ſervant,

JOHN WHITEHURST.

Pleaſe to obſerve, that the circumſtances attending this water-work, require a particular attention, and are as follows (ſee TAB. VII. fig. A.): (A) repreſents the ſpring or original reſervoir, whoſe upper ſurface coincides with the horizontal line BC, and the bottom of the reſervoir K. D the main pipe, $1\frac{1}{2}$ inch diameter, and nearly two hundred yards in length. E a branch pipe, of the former dimenſions, for the ſervice of the kitchen offices. Now it is to be obſerved, that the kitchen offices are ſituated at leaſt eighteen or twenty feet below the ſurface of the reſervoir A, and that the cock F is about ſixteen feet below it. G repreſents a valve-box, *g* the valve, H an air-veſſel, *oo* the ends of the main-pipe inſerted into H, and bending downwards, to prevent the air from being driven out when the water is forced into it, w the ſurface of the water. Now it is well known, that water diſcharged from an aperture, under a preſſure of ſixteen feet perpendicular height, moves at the rate of thirty-two feet in a ſecond of time; therefore ſuch will be the velocity of the water from the cock F. And although the aperture of the cock F is not equal to the diameter of the pipe D, yet the velocity of the water contained in it will be very conſiderable: conſequently, when a column of water, two hundred yards in length, is thus put into motion, and ſuddenly ſtopped by the cock F, its momentous force will open the valve *g*, and condenſe the air in H, as often as water is drawn from F. In what degree the air is thus condenſed, is needleſs to ſay in the inſtance before us; therefore I ſhall only obſerve, that it was ſufficiently condenſed to force out the water into the reſervoir K, and even to burſt the veſſel H, in a few months after it was firſt conſtructed, though apparently very firm, being made of ſheet lead, about nine or ten pounds weight to a ſquare foot. From whence it ſeems reaſonable to infer, that the momentous force is much ſuperior to the ſimple preſſure of the column IK; and therefore equal to a greater reſiſtance (if required) than a preſſure of four or five feet, perpendicular height. It ſeems neceſſary further to obſerve, that the conſumption of water in the kitchen offices is very conſiderable; that is, that water is frequently drawing from morning till night all the days of the year.

XXV. *Extract of a Letter from Mr.* Lexel *to Dr.* Morton. *Dated* Peterſburg, June 14, 1774.

Redde, Mar. 16, 1775.

AS I propoſe to make ſome reſearches concerning the difference of the meridians of the principal Obſervatories of Europe, which I am perſuaded can beſt be aſcertained by the occultations of the fixed ſtars by the Moon; it would be of great ſervice to me to be furniſhed with the obſervations that have been made, or that will be made, this year, of the occultations of α or of γ Tauri by the Moon. I beg, therefore, SIR, you will pleaſe to deſire Mr. MASKELYNE to communicate them to me, towards the beginning of the next year, directed to Mr. EULER, ſecretary of our Academy. It would alſo be of great uſe to me to have the obſervation of the occultation of the Pleiades by the Moon the 15th of March, 1766, in caſe it has been taken at Greenwich.

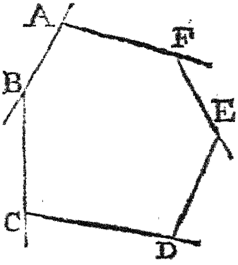
Here are ſome obſervations of Mr. Wargentin, of the occultations of α and γ Tauri.

		h	′	″		
1773, Nov.	1	11	56	12	Emerſion of α, uncertain to ſome ſeconds.	
1774, Jan.	22	6	0	26½	Immerſion of the eye of ♉,	both very certain.
		7	15	51	Emerſion,	
Feb.	18	6	39	51	Immerſion of γ, very certain.	
		7	19	33	Emerſion, within two ſeconds.	

The following are my obſervations.

		h	′	″		
1773, Nov.	1	12	56	47	Emerſion of α almoſt certain; the immerſion was not obſerved on account of clouds.	
1774, Jan.	22	7	2	52	Immerſion,	both certain.
		8	20	44	Emerſion,	
April	14	8	28	34	Immerſion of α, very certain.	
		9	3	20	Emerſion of the ſame.	
	15	9	32	0	Immerſion of FLAMSTEAD's 115 in ♉.	
	16	10	21	31	Immerſion of a ſtar of the 6th magnitude in ♊.	
May	22	13	2	20	Immerſion of m Virginis, very certain.	

I have lately diſcovered two curious theorems, which I ſhall here communicate to the Royal Society.



THEOREM.

Let A, B, C, D, E, F, be a polygon whoſe ſides are named a, b, c, d, e, f; and the exterior angles α, β, γ, δ, ε, ζ, ſo that the ſide a be placed between the angles α and β, b between β, γ, &c.

$$1.\ \overline{a \times \text{ſin.}\,\alpha} + \overline{b \times \text{ſin}\,(\alpha+\beta)} + \overline{c \times \text{ſin.}\,(\alpha+\beta+\gamma)} + \overline{d \times \text{ſin.}\,(\alpha+\beta+\gamma+\delta)} + \overline{e \times (\text{ſin.}\,\alpha+\beta+\gamma+\delta+\varepsilon)} + \overline{f \times \text{ſin.}\,(\alpha+\beta+\gamma+\delta+\varepsilon+\zeta)} = 0.$$

$$2.\ \overline{a \times \text{coſin.}\ \alpha} + \overline{b \times \text{coſ.}\ (\alpha + \beta)} + \overline{c \times \text{coſ.}\ (\alpha + \beta + \gamma)} + \overline{d \times \text{coſ.}\ (\alpha + \beta + \gamma + \delta)} + \overline{e \times \text{coſ.}\ (\alpha + \beta + \gamma + \delta + \varepsilon)} + \overline{f \times \text{coſ.}\ \alpha + \beta + \gamma + \delta + \varepsilon + \zeta)} = 0.$$

In fact it is ſin. $(\alpha + \beta + \gamma + \delta + \varepsilon + \zeta) =$ ſin. $360^\circ = 0.$ and coſ. $(\alpha + \beta + \gamma + \delta + \varepsilon + \zeta) = + 1.$; but in order to give the ſame form to the two expreſſions, I rather choſe to repreſent them as I have done. By means of theſe two theorems the ſolution of polygons will be as eaſy as that of triangles by common trigonometry.

XXVI. *An Inveſtigation of a general Theorem for finding the Length of any Arc of any Conic Hyperbola, by Means of Two Elliptic Arcs, with ſome other new and uſeful Theorems deduced therefrom.* By John Landen, *F. R. S.*

Redde, Mar. 23, 1775.

IN a paper, which the Society did me the honour to publiſh in the Philoſophical Tranſactions for the year 1771, I announced, that I had diſcovered a general theorem for finding the length of any arc of any conic hyperbola, by means of two elliptic arcs; and I promiſed to communicate the inveſtigation of ſuch theorem. I now purpoſe to perform my promiſe; and, being pleaſed with the diſcovery (by which we are enabled to bring out very elegant concluſions in many intereſting enquiries, as well mechanical as purely geometrical), I cannot but flatter myſelf, that what I am about to communicate will be acceptable to gentlemen who are curious in ſuch inquiries.

1. From the theorem taken notice of in Art. 1. of the paper I have juſt now mentioned, it follows, that in the hyperbola AD (TAB. VII. fig. 1.), if the ſemi-tranſverſe axis AC be $= m - n$; the ſemi-conjugate $= 2 \times \overline{mn}|^{\frac{1}{2}}$; and the perpendicular CP, from the center C upon the tangent DP, $= \overline{\overline{m-n}|^2 - t^2}|^{\frac{1}{2}}$; the difference (DP − AD) between the ſaid

tangent DP and the arc AD will be equal to the fluent of $\left.\frac{\overline{m-n}|^{2}-t^{2}}{\overline{m+n}|^{2}-t^{2}}\right|^{\frac{1}{2}}\times\dot{t}$.

2. It is well known, that in any ellipſis whoſe ſemi-tranverſe axis is m, and ſemi-conjugate n; if x be the abſciſſa, meaſured from the center upon the tranſverſe axis, and z the arc between the conjugate axis and the ordinate correſponding to x, $\left.\frac{m^{2}-gx^{2}}{m^{2}-x^{2}}\right|^{\frac{1}{2}}\times\dot{x}$ will be $=\dot{z}$, g being $=\frac{m^{2}-n^{2}}{m^{2}}$.

Hence, $\left.\frac{\overline{m+n}|^{2}-t^{2}}{\overline{m-n}|^{2}-t^{2}}\right|^{\frac{1}{2}}\times\dot{t}$ being $=\left.\frac{\overline{m+n}|^{2}-t^{2}}{\overline{m+n}|^{2}-\overline{\frac{m+n}{m-n}}|^{2}t^{2}}\right|^{\frac{1}{2}}\times\frac{m+n}{m-n}\dot{t}$, it appears, that in the ellipſis aed (fig. 2.) whoſe ſemi-tranſverſe axis cd is $=m+n$, ſemi-conjugate ca $=2\times\overline{mn}|^{\frac{1}{2}}$, and abſciſſa cb (correſponding to the ordinate be) $=\frac{m+n}{m-n}t$; the arc ae is equal to the fluent of $\left.\frac{\overline{m+n}|^{2}-t^{2}}{\overline{m-n}|^{2}-t^{2}}\right|^{\frac{1}{2}}\times\dot{t}$.

3. In the ellipſis aefd (fig. 3.), the ſemi-tranſverſe axis cd being $=m$; the ſemi-conjugate ca $=n$; and the abſciſſa cb (correſponding to the ordinate be) $=x$; if ep, the tangent at e, intercepted by a perpendicular (cp) drawn thereto from the center c, be denoted by t; $gx\times\left.\frac{m^{2}-x^{2}}{m^{2}-gx^{2}}\right|^{\frac{1}{2}}$ (as is well known) will be $=t$, g being as in the preceding article.

Hence $x^2 = \frac{m^2g+t^2}{2g} - \frac{\overline{\overline{m^2-n^2}\big|^2 - 2\times\overline{m^2+n^2}\times t^2+t^4}\Big|^{\frac{1}{2}}}{2g}$. From which equation, by taking the fluxions, we have,

$$x\dot{x} = \frac{t\dot{t}}{2g} + \frac{\overline{m^2+n^2}\times t\dot{t} - t^3\dot{t}}{2g\times\overline{\overline{m^2-n^2}\big|^2 - 2\times\overline{m^2+n^2}\times t^2+t^4}\Big|^{\frac{1}{2}}}$$

$= \frac{t\dot{t}}{2g} + \frac{\overline{m^2+n^2}\times t\dot{t} - t^3\dot{t}}{2g\times\overline{\overline{\overline{m-n}\big|^2 - t^2}\times\overline{\overline{m+n}\big|^2-t^2}}\Big|^{\frac{1}{2}}}$. But $\dot{z}$ being $= \overline{\frac{m^2-gx^2}{m^2-x^2}}\Big|^{\frac{1}{2}}\times\dot{x}$, as obſerved in the preceding article, it appears that $\frac{g}{t}\times x\dot{x}$ is $= \dot{z}$. It is obvious, therefore, that

$$\dot{z} \text{ is } = \tfrac{1}{2}\dot{t} + \tfrac{1}{2}\times\frac{\overline{m^2+n^2}\times\dot{t} - t^2\dot{t}}{\overline{\overline{\overline{m-n}\big|^2-t^2}\times\overline{\overline{m+n}\big|^2-t^2}}\Big|^{\frac{1}{2}}}$$

$$= \tfrac{1}{2}\dot{t} + \tfrac{1}{4}\times\frac{\overline{m-n}\big|^2\times\dot{t} - t^2\dot{t}}{\overline{\overline{\overline{m-n}\big|^2-t^2}\times\overline{\overline{m+n}\big|^2-t^2}}\Big|^{\frac{1}{2}}} + \tfrac{1}{4}\times\frac{\overline{m+n}\big|^2\times\dot{t} - t^2\dot{t}}{\overline{\overline{\overline{m-n}\big|^2-t^2}\times\overline{\overline{m+n}\big|^2-t^2}}\Big|^{\frac{1}{2}}}$$

$= \tfrac{1}{2}\dot{t} + \tfrac{1}{4}\times\overline{\frac{\overline{m-n}\big|^2-t^2}{\overline{m+n}\big|^2-t^2}}\Big|^{\frac{1}{2}}\times\dot{t} + \tfrac{1}{4}\times\overline{\frac{\overline{m+n}\big|^2-t^2}{\overline{m-n}\big|^2-t^2}}\Big|^{\frac{1}{2}}\times\dot{t}$. From whence taking the fluents by the theorems in art. 1. and 2. we have $z = \text{ae}$ (fig. 3.) $= \tfrac{1}{2}t + \frac{\text{DP}-\text{AD}}{4}$ (fig. 1.) $+ \frac{\text{ae}}{4}$ (fig. 2.) conſequently the hyperbolic arc AD is $= \text{DP} + \text{ae} + 2t - 4\text{ae}$. Thus, beyond my expectation, I find, that the hyperbola may in general be rectified by means of two ellipſes.

Writing E and F for the quadrantal arcs ad, ad, (fig. 2. and 3.) reſpectively, and L for the limit of the difference DP − AD, whilſt the point of contact (D) is ſuppoſed to be carried to an infinite diſtance from the vertex A of the hyperbola (fig. 1.), we find

$$2\text{F} - \text{E} = \text{L},$$

the value of ae being $= \tfrac{1}{2}\text{F} + \tfrac{1}{2}m - \tfrac{1}{2}n$ when t is $= m - n$;

that is, when e coincides with d (fig. 2.), and P with C (fig. 1.), by what I have proved in the before mentioned paper, art. 10.

4. From what is done above, the following uſeful theorems are deduced.

THEOREM I.

The fluent of $\frac{1}{2}a^{\frac{1}{2}}z^{-\frac{1}{2}}\dot{z}\times\overline{\frac{\frac{b^2}{a}+z}{a-z}}\Big|^{\frac{1}{2}}$ is $=$ de.

THEOREM II.

The fluent of $\frac{1}{2}a^{\frac{1}{2}}z^{-\frac{1}{2}}\dot{z}\times\overline{\frac{a-z}{\frac{b^2}{a}+z}}\Big|^{\frac{1}{2}}=\overline{\frac{2a^2}{b^2}+1}\big|.\mathrm{de}-\overline{\frac{2a^2}{b^2}+2}\big|.\mathrm{ef}.$

THEOREM III.

The fluent of $\frac{\frac{1}{2}a^{\frac{1}{2}}z^{\frac{1}{2}}\dot{z}}{\overline{b^2+2kz-z^2}|^{\frac{1}{2}}}=2\,\mathrm{ef}-\mathrm{de}=2\,\mathrm{F}-\mathrm{E}+\mathrm{AD}-\mathrm{DP}.$

THEOREM IV.

The fluent of $\frac{\frac{1}{2}a^{-\frac{1}{2}}b^2z^{-\frac{1}{2}}\dot{z}}{\overline{b^2+2kz-z^2}|^{\frac{1}{2}}}=2\times\overline{\mathrm{de}-\mathrm{ef}}.$ N.B. $k=\frac{a^2-b^2}{2a}$.

Theſe theorems ſtill refer to fig. 1. 2. 3.; but now the values of the ſeveral lines therein (being not as before) are as here ſpecified; *videlicet*,

Fig. 1. In the hyperbola AD, the ſemi-tranſverſe axis AC is now $= a$; the ſemi-conjugate $= b$; the perpendicular CP, from the center C upon the tangent DP, is $=\overline{az}|^{\frac{1}{2}}$ the ſaid tangent DP $=\frac{a}{z}\Big|^{\frac{1}{2}}\times\overline{b^2+2kz-z^2}|^{\frac{1}{2}}$; and the abſciſſa CB (correſponding to the ordinate BD) is $=\frac{a^{\frac{3}{2}}}{z^{\frac{1}{2}}}\times\overline{\frac{az+b^2}{a^2+b^2}}\Big|^{\frac{1}{2}}$

Fig. 2. In the ellipſis aed, the ſemi-tranſverſe axis cd is $=\overline{a^2+b^2}|^{\frac{1}{2}}$; the ſemi-conjugate ca$=b$; the abſciſſa cb$=\overline{\frac{a^2+b^2}{a}}|^{\frac{1}{2}}\times\overline{a-z}|^{\frac{1}{2}}$; and the ordinate be$=b\times\overline{\frac{z}{a}}|^{\frac{1}{2}}$.

Fig. 3. In the ellipſis aefd, the ſemi-tranſverſe axis cd is$=\frac{1}{2}.\overline{a^2+b^2}|^{\frac{1}{2}}+\frac{1}{2}a$; the ſemi-conjugate ca$=\frac{1}{2}.\overline{a^2+b^2}|^{\frac{1}{2}}-\frac{1}{2}a$; the tangents ep, fq, intercepted by perpendiculars (cp, cq) drawn thereto from the center c, each$=a^{\frac{1}{2}}\times\overline{a-z}|^{\frac{1}{2}}$; and the abſciſſa (c b′ or c b″) on cd, correſponding to the point e or f, of the curve is determined by the expreſſion

$$\frac{\overline{\overline{a^2+b^2}|^{\frac{1}{2}}+a-z\mp\overline{z^2+\frac{b^2}{a}z}|^{\frac{1}{2}}}|^{\frac{1}{2}}}{2^{\frac{1}{2}}+\overline{a^2-b^2}|^{\frac{1}{2}}}\times \text{cd}.$$

The quadrantal arc ad (fig. 2.) is denoted by E; and the quadrantal arc ad (fig. 3.) is denoted by F. L the limit of DP$-$AD (fig. 1.) is $=2\text{F}-\text{E}$.

From what is now done, I might proceed to deduce many other new theorems, for the computation of fluents; but I ſhall, at preſent, decline that buſineſs: and, after giving a remarkable example of the uſe of theorem 4. in computing the deſcent of a heavy body in a circular arc, conclude this paper with a few obſervations relative to the contents of the preceding articles.

5. Let lpqn (fig. 4.) be a ſemi-circle perpendicular to the horizon, whoſe higheſt point is l, loweſt n, and center m. Let ps, qt, parallel to the horizon, meet the diameter lmn in s and t; and let the radius lm (or mn) be denoted by r; the height ns by d; and the diſtance

ſt by x. Then, putting b for ($16\frac{1}{12}$ feet) the ſpace a heavy body, deſcending freely from reſt, falls through in one ſecond of time; and ſuppoſing a pendulum, or other heavy body, deſcending by its gravity from p, along the arc pqn, to have arrived at q; the fluxion of the time of deſcent will be $=\frac{\frac{1}{2}rb^{-\frac{1}{2}}x^{-\frac{1}{2}}\dot{x}}{\overline{2rd-d^2-2.\overline{r-d}.x-x^2}|^{\frac{1}{2}}}$. The fluent whereof, or the time of deſcent from p to q is (by theor. 4. of the preceding article) $=\frac{2r}{b^{\frac{1}{2}}\times\overline{2r-d}}\times$ de−ef. a (in that theorem) being taken $=d^{\frac{1}{2}}$, $b=\overline{2r-d}|^{\frac{1}{2}}$, cb (fig. 2.) $=\overline{\frac{2r}{d}}|^{\frac{1}{2}}\times\overline{d-x}|^{\frac{1}{2}}$, and ep, fq, (fig. 3.) each $=\overline{d-x}|^{\frac{1}{2}}$. Hence it appears, that the whole time of deſcent from p to n is $=\frac{2r}{b^{\frac{1}{2}}\times\overline{2r-d}}\times\overline{\mathrm{E}-\mathrm{F}}$; when, in fig. 2. and 3. the ſemi-axes are taken according to the values of a and b juſt now ſpecified.

6. If pqn be a quadrant; that is, if d be $=r$, the whole time of deſcent from p to n will be $=\frac{2}{b^{\frac{1}{2}}}\times\overline{\mathrm{E}-\mathrm{F}}$, by the above theorem. Which time, by what I have ſhewn in the Philoſ. Tranſact. for 1771, is $=\frac{1}{b^{\frac{1}{2}}}\times\overline{\frac{1}{2}\mathrm{E}+\frac{1}{2}\sqrt{\mathrm{E}^2-2c}}$, c being $\frac{1}{4}$ of the periphery of the circle whoſe radius is r. Conſequently, $\frac{2}{b^{\frac{1}{2}}}\times\overline{\mathrm{E}-\mathrm{F}}$ being found $=\frac{1}{b^{\frac{1}{2}}}\times\overline{\frac{1}{2}\mathrm{E}+\frac{1}{2}\sqrt{\mathrm{E}^2-2c}}$, we find from that equation $\mathrm{F}=\frac{3}{4}\mathrm{E}-\frac{1}{4}\sqrt{\mathrm{E}^2-2c}$, where E is the quadrantal arc of the ellipſis, whoſe ſemi-tranſverſe and ſemi-conjugate axes are $\overline{2r}|^{\frac{1}{2}}$ and $r^{\frac{1}{2}}$; and F the quadrantal.

drantal arc of another ellipſis, whoſe ſemi-tranſverſe and ſemi-conjugate axes are $\overline{\frac{r}{2}}|^{\frac{1}{2}} + \frac{1}{2}r^{\frac{1}{2}}$ and $\overline{\frac{r}{2}}|^{\frac{1}{2}} - \frac{1}{2}r^{\frac{1}{2}}$.

Before Mr. MACLAURIN publiſhed his excellent Treatiſe of Fluxions, ſome very eminent mathematicians imagined, that the *elaſtic curve* could not be conſtructed by the quadrature or rectification of the conic ſections. But that gentleman has ſhewn, in that treatiſe, that the ſaid curve may in every caſe be conſtructed by the rectification of the hyperbola and ellipſis; and he has obſerved, that, by the ſame means, we may conſtruct the curve along which, if a heavy body moved, it would recede equally in equal times from a given point. Which laſt mentioned curve Mr. JAMES BERNOULLI conſtructed by the rectification of the elaſtic curve, and Mr. LEIBNITZ and Mr. JOHN BERNOULLI by the rectification of a geometrical curve of a higher kind than the conic ſections. It is obſervable, that Mr. MACLAURIN's method of conſtruction juſt now adverted to, though very elegant, is not without a defect. The difference between the hyperbolic arc and its tangent being neceſſary to be taken, the method always fails when ſome principal point in the figure is to be determined; the ſaid arc and its tangent then both becoming infinite, though their difference be at the ſame time finite. The contents of this paper, properly applied, will evince, that both the *elaſtic curve* and the *curve of equable receſs from a given point* (with many others) may be conſtructed by the rectification of the ellipſis only, without failure in any point

XXVII. *Obſervations made at* Chiſlehurſt, *in* Kent, *in the Year* 1774. *By the Rev.* Francis Wollaſton, *LL.B. F.R.S.*

Redde, Mar. 16, 1775.

HAVING now compleated my original deſign, and kept my clock going for a third year, without the leaſt touch of the oil, or any alteration whatſoever, I preſume the reſult of my obſervations to aſcertain the rate of its going, may not be an unacceptable addition to the former papers on that ſubject, delivered to this Society. The regular difference between the ſummer and winter months, and ſome degree of ſimilarity between thoſe differences, ſeems to ſhew a regularity in the cauſe. What that may be, is not fully to be aſcertained hereby; though it ſeems to have been difference of moiſture, rather than of heat. By comparing theſe three laſt years with that which I firſt gave, when the clock was in ſome degree foul, it ſeems as if it were moſt affected when the work is clean. Yet is not that quite certain; for the differences, which decreaſing gradually in the following table, would juſtify this concluſion, it may be obſerved, increaſe again in the laſt inſtance.

The

		per Day. ″	Diff. ″
The firſt greateſt loſs was	Dec. 1770 or	− 1,9	
The next greateſt gain	June 1771	+ 2,4	4,3
The next greateſt loſs	Oct. 1771	− 1,9	4,3

The clock was cleaned November 1771, and might not be reckoned to get to any ſtated rate till the beginning of the next year; after which,

			Diff.
The firſt greateſt loſs was	Feb. 1772 or	− 0,86	
The next greateſt gain	July 1772	+ 7,83	8,69
The next greateſt loſs	Jan. 1773	− 0,09	7·92
The next greateſt gain	Aug. 1773	+ 6,17	6,26
The next leaſt gain	Feb. 1774	+ 0,30	5,87
The next greateſt gain	Aug. 1774	+ 4,95	4,65
The next greateſt loſs to the end of that year,	Dec. 1774	− 0,90	5,85

Hereby July and Auguſt appear to be the months for greateſt acceleration, and January and February for retardation; contrary to the affection of metalline rods, but agreeable to the effect to be expected from moiſture upon wood. Yet this difference is not ſo great in any degree, nor (what is more material to obſervation) by any means ſo ſudden in its changes, as what is occaſioned by heat upon metals. And even this, perhaps, might be obviated by a ſtrong coat of varniſh on the rod, or ſome preparation of the wood itſelf. One thing it may be proper to mention, as an accidental experience I have had the laſt year; that a clock ſo fixed, with a pendulum of ſo ſimple conſtruction, is not eaſily affected by any tremulous motion of the building to which it is faſtened. In the months of March, April, and part of May, I had occaſion to make alterations in the top of my houſe, in order to gain more rooms in it; and notwithſtanding the great jarring neceſſarily conſequent upon taking off the old rafters, and laying on a new leaded roof, and new joiſts and floor over the obſervatory itſelf, the clock ſeems not to have been diſordered at all by it. Between February 7th and 20th there will appear an error in the calculation of gain, to any one who ſhall take the trouble to examine it: not that I believe there is really any error in it; but by an accident in winding the clock (not having put down the ſpring ſufficiently, which is intended to keep on the motion of the wheels,) there were 6″ loſt, as appeared by the aſſiſtant clock, which had been ſet with it juſt before. Theſe 6″ being allowed for, will reduce the loſs of $\overset{\prime\prime}{2},1$ to a gain of $\overset{\prime\prime}{3},9$ in that interval of thirteen days, $-\overset{\prime\prime}{2},1+\overset{\prime\prime}{6}=+\overset{\prime\prime}{3},9$. In the months of February, March, and April, I was frequently from home; ſo that the ſtate of the thermometer and barometer, if I were to ſet them down, would be very imperfect. In the other months they are more compleat; yet there may, perhaps, in my abſence, have been ſome days in them either higher or lower than what are here given.

	Clock + too faſt. — too ſlow. for mean ſol. time.	Numb. of Days.	Gain + or Loſs —	Rate per Day.	Throwing out	
					South ſide.	North ſide.
	′ ″		″	″	° ′	° ′
1773. Dec. 27	Clock + 32 19,7					
		7	+ 7,2	+ 1,03	1 25	1 28
1774. Jan. 3	+ 32 26,9					
		35	+ 14,8	+ 0,42		
Feb. 7	+ 32 41,7				1 27	1 29
13	Loſs in winding 6″					
		13	+ 3,9	+ 0,30		
20	+ 32 39,6				1 35	1 38
		20	+ 6,0	+ 0,30		
Mar. 12	+ 32 45,6				1 28	1 31
		8	+ 2,4	+ 0,30		
20	+ 32 48,0				1 40	1 43
		13	+ 5,1	+ 0,39		
April 2	+ 32 53,1				1 35	1 40
		29	+ 52,0	+ 1,79		
May 1	+ 33 45,1				1 36	1 40
		25	+ 70,1	+ 2,80		
26	+ 34 55,2				1 40	1 43
		13	+ 45,4	+ 3,49		
June 8	+ 35 40,6				1 38	1 41
		14	+ 49,9	+ 3,57		
22	+ 36 30,5					
		9	+ 37,3	+ 4,14	1 36	1 40
July 1	+ 37 7,8					
		31	+ 134,7	+ 4,34	1 37	1 40
Auguſt 1	+ 39 22,5					
		18	+ 86,9	+ 4,83		
19	+ 40 49,4					
		15	+ 74,3	+ 4,95	1 32	1 35
Sept. 3	+ 42 3,7					
		9	+ 33,3	+ 4,70	1 35	1 38
12	+ 42 37,0					
		21	+ 68,3	+ 3,25	1 35	1 38
Oct. 3	+ 43 45,3					
		12	+ 23,9	+ 1,99	1 33	1 36
15	+ 44 9,2					
		14	+ 28,7	+ 2,05	1 30	1 33
29	+ 44 37,9					
		14	+ 29,8	+ 2,11	1 30	1 33
Nov. 12	+ 45 7,7					
		23	+ 19,4	+ 0,84		
Dec. 5	+ 45 27,1					
		8	— 1,7	— 0,21		
13	+ 45 25,4					
		11	— 6,9	— 0,63		
24	+ 45 18,5				1 20	1 23
		8	— 7,2	— 0,90		
1775. Jan. 1	+ 45 11,3					

1774.

		Thermometer without doors expoſed to the North.			Therm. near the clock.	Barometer on the ground floor.	Hygrom. near the clock.
		Hor. 8 A. M	Hor. 2. P. M.	Hor. 11. P. M.	Hor. 9. A. M.		
1774.							
Jan.	Higheſt	48	50	48	47	29,85	70
	Loweſt	23,5	29	23	31	28,67	34
May	Higheſt	60	67	53	57	29,98	23
	Loweſt	45	45,5	40	47	29,10	10
June	Higheſt	70	74	60	65	30,11	20
	Loweſt	51	55	47	51	29,24	9
July	Higheſt	72	82	60	65	30,14	19
	Loweſt	57	57	50	53	29,52	7
Auguſt	Higheſt	70	76	63	65	30,12	34
	Loweſt	57	61	50	53	29,32	5
Sept.	Higheſt	70	72	60	63	30,06	32
	Loweſt	49	51	46	51	29,915	11
Oct.	Higheſt	56	64	54	55	30,37	27
	Loweſt	39	45	39	44	29,23	13
Nov.	Higheſt	54	59	51	49	30,06	42
	Loweſt	31,5	33	31	34	29,02	20
Dec.	Higheſt	50	53	51	45	30,54	70
	Loweſt	21	30,5	25	30	28,95	27

Accidents of weather and various avocations have prevented me from making any other obſervations in the courſe of the laſt year; excepting the ſecond diſappearance of Saturn's ring, and re-appearance of it again, both of which I was fortunate enough to obſerve. I had ſeen the ring many times after its firſt re-appearance; and obſerved it to be leſſening again, till it was become but a mere a mere thread of light, Monday, April 4, though certainly viſible then. Tueſday, April 5, the evening was very clear, yet no ring could I perceive with my 3½ feet achromatic teleſcope; nor from that time did I ſee any thing of it (but, during part of the interval, the appearance of a dark belt acroſs the planet,) till Thurſday, June 30, when I thought I ſaw the preceding *anſa*. Saturday, July 2, I am ſure I ſaw the whole ring again, as a thread of light; and as the preceding *anſa*, or end of that thread, appeared larger than the ſubſequent one, it probably was viſible, and not only a deception, when I fancied I perceived it before. In theſe obſervations it deſerves to be remarked, that the magnifying power of 100 ſeemed, from its brightneſs, to ſhew the thread of the ring more viſibly than 150.

Chiſleſhurſt,
Jan. 1775.

FRANCIS WOLLASTON.

XXVIII. *Of Triangles deſcribed in Circles and about them.*

By John Stedman, *M. D.*

Redde, March 23, 1775.

P R O P O S I T I O N I.

An equilateral triangle inſcribed within a circle is larger than any other triangle that can be inſcribed within the ſame circle.

LET ABC (TAB. VIII. fig. 1.) be an equilateral triangle, inſcribed in the circle ADCB; and let ADE be a triangle ſuppoſed larger than ABC. Let ADE be drawn with one of its angles at the ſame point with one of the angles of the equilateral triangle, ſuppoſe at A, and then its other two angles will fall either on the ſegments ADB and AEC, or one of the angles will on the ſegment BC. Firſt, let one of its angles fall at D, between A and B; and the other at E, between A and C, and draw the line BE. In the triangles ABC, ABE, the triangle ABF is common, and the two remaining triangles BFC, AFE, are ſimilar; for the angle AFE is equal to its oppoſite angle BFC; and the two angles EAC, EBC, are equal, being ſubtended by the ſame ſegment EC, and ſo the two remaining angles

AEF, BCF, muſt be equal; wherefore the ſides are proportional, and BC and AE, ſubtending equal angles muſt be homologous; but BC is equal to AC, which is greater than AE; conſequently the triangle BFC is greater than AFE, and ſo the equilateral triangle ABC is greater than the triangle ABE. In the ſame manner, the triangle ABE may be proved greater than ADE; for AHE is common, and the two triangles ADH, BHE, are ſimilar, and their ſides proportional; and AD and BE, ſubtending equal angles, muſt be homologous; but BE is greater than BC, which is equal to AB, and that again greater than AD; conſequently BE is greater than AD, and the whole triangle AEB greater than AED; and ſo the equilateral triangle muſt, *à fortiori*, be greater than AED. *Q. E. D.*

Next, let the triangle ADE be ſuppoſed greater than the equilateral triangle ABC, and let the angle ADE fall ſomewhere in the ſegment BDC, (ſee fig. 2.) ſo that the ſegment EC may be greater than BD; for if it were not, the angle AED being applied to any of the angles of the equilateral triangle, the demonſtration would become the ſame as in the firſt caſe: wherefore the ſegments AEC, BDC, being equal, and BD being leſs than EC, AE muſt be leſs than DC. Draw the right line DC, and then in the two triangles ADC, ADE, the triangle AFD is common, and the two triangles AFE, DFC, are equiangular and ſimilar, and the ſides AE, DC, ſubtending equal angles, are homologous; but DC is greater than AE; ſo the triangle DFC is greater than the triangle AFE, and the whole triangle ADC is greater than ADE; but the equilateral triangle may be proved greater than ADC from the firſt caſe, and conſequently greater than ADE. *Q. E. D.*

PROPOSITION II.

An equilateral triangle deſcribed about a circle is leſs than any other triangle that can be deſcribed about the ſame circle. Fig. 3.

LET the equilateral triangle ABC be deſcribed about the circle HIK, and let the triangle BDG be ſuppoſed leſs than the equilateral triangle. Draw the line AF parallel to BC, then the triangles AFE, EGC, are ſimilar; for the oppoſite angles AEF, GEC, are equal, as likewiſe the angle AFE to the angle EGC; the lines AF and GC being parallel, and falling upon the ſame line FG, the angles AFE and EGC are therefore equal, and the ſides AE, EC, ſubtending equal angles, are homologous; but the ſide of the equilateral triangle AC being equally divided at I, the line AE is greater than EC, and conſequently the triangle AFE is larger than the triangle EGC; and the triangle DAE much larger than EGC: therefore, in the triangles DBG and ABC the part ABGE being common the whole triangle DBG is larger than the equilateral triangle. *Q. E. D.*

Whatever other triangles can be deſcribed about a circle, may be demonſtrated to be larger than an equilateral triangle deſcribed about the ſame circle, upon the ſame principles as the preceding.

PROPOSITION III.

The ſquare of the ſide of an equilateral triangle inſcribed in a circle is equal to a rectangle under the diameter of the circle, and a perpendicular let fall from any angle of the triangle upon the oppoſite ſide. Fig. 4.

THE two triangles ADC, AEC, are equi-angular and ſimilar, the angles ACD, AEC, being both right, and that at A common; therefore AD : AC :: AC : AE, and $AC^2 =$ AD × AE. *Q.E.D*(a).

The ſquare of one ſide of the triangle being compleated ſo as to include the triangle, I ſay, that part of the ſide of the ſquare that falls within the circle is equal to the radius; and the other part, lying without the circle, is equal to the radius *minus* twice the portion lying between the ſide of the ſquare, and the circumference of the circle; or is equal to that part of the radius that lies between the centre and the ſide of the ſquare *minus* the remainder of the radius; that is CL is equal to the radius, and LI=KG—2MG; or LI=KM-MG, FG being parallel to BC, and conſequently perpendicular to IC, muſt divide the chord LC in two equal parts; ſo that MC being equal to KE, LC muſt be equal to 2KE; but KE (by EUCL. l. XIII. pr. 12. cor. 2. Clav.) is equal to KD; therefore LC=KD the radius. The ſide of the ſquare IC being equal to BC is likewiſe equal to NM; but LC being equal to KG, the remaining part LI muſt be equal to NK—MG; or to KM—MG. *Q. E. D.*

(a) And univerſally, a perpendicular being drawn from any angle of a right-lined triangle to the oppoſite ſide, the rectangle under the two ſides which contain that angle, is equal to the rectangle under the perpendicular and the diameter of the circumſcribed circle. (See TAB. VIII. fig. 5.)

From A, one angle of the triangle BAC, draw AE perpendicular to the ſide BC. Round the triangle BAC deſcribe a circle, and draw the diameter AD. I ſay, the rectangles AC × AB, AE × AD, are equal. Join DB. The angle DBA is a right angle. Therefore it is equal to the right angle CEA. The angles at the circumference ACE, ADB, are equal, becauſe they ſtand upon the ſame arc AB. Therefore the two triangles ACE, ADB, are ſimilar, and AC : AE = AD : AB. Therefore AC × AB = AE × AD. *Q. E. D.* S. HORSLEY.

Fig. 1.

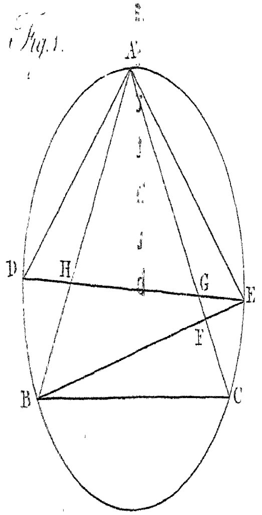


Fig. 2.

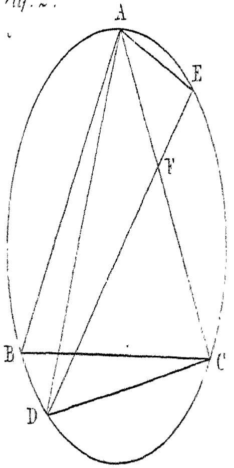


Fig. 3.

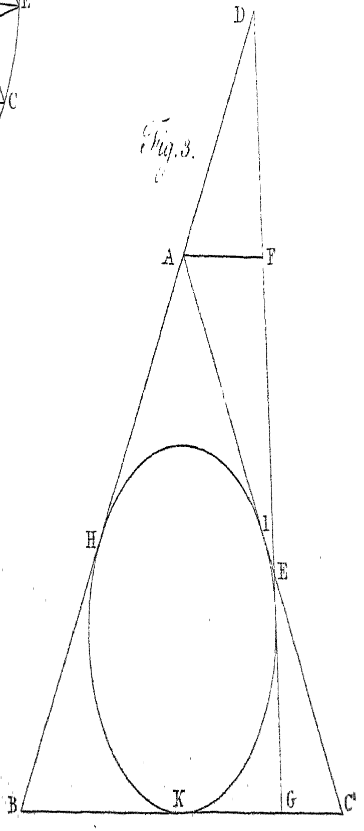


Fig. 4.

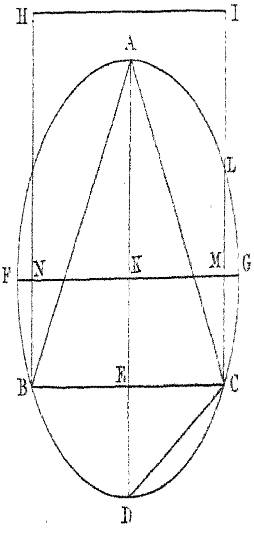
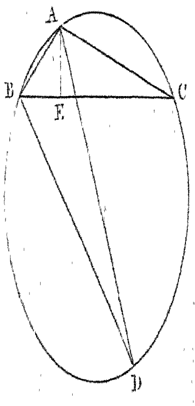


Fig. 5.



Basire sc.

XXIX. *De Polygonis Areâ vel Perimetro maximis et minimis, inſcriptis Circulo, vel Circulum circumſcribentibus.* *Auctore* S. Horſley, *LL.D. R.S. Sec.*

Redde, May 19, 1775.

THEOREMA I. (TAB. IX.)

Si linea recta arcum circularem contingentibus duabus interceptum contingat, ſegmentum ejus, contingentibus primo poſitis interceptum, in contactûs ſui puncto vel æqualiter vel inæqualiter diviſum eſt, prout arcus ipſe circularis æqualiter vel inæqualiter in eodem puncto diviſus eſt. Segmentaque arcûs (inæqualiter ſcilicet diviſi) et rectæ contingentis majora et minora ab iiſdem ſunt partibus mutui contactûs.

ARCUM circularem AEC, contingentibus duabus, AB, CD, interceptum, recta BD in puncto E contingat. Recta vero BD contingentibus primo poſitis, AB, CD, in punctis B, D, occurrat. Dico rectam BD in puncto E vel æqualiter vel inæqualiter diviſam, prout arcus AEC æqualiter vel inæqualiter in eodem puncto E diviſus eſt. Primo puta arcum AC in puncto E æqualiter diviſum. Dico igitur et rectam BD, in puncto E, bifariam ſecari. Circuli AEC centrum eſto F. Jungantur FE, FB, FD, quarum FB, FD, circuli peripheriæ in punctis G, H, occurrant. Rectæ FB, FD arcus AE, CE bifariam dividunt. Arcus igitur GE, HE, arcuum AE, CE ſemiſſes. Æquales igitur GE, EH, propter AE, EC, ex hypotheſi, æquales. Quare anguli BFE, DFE æquales. Æquales autem anguli BEF, DEF: rectus enim uterque. In triangulis igitur BFE, DFE, quæ latus commune habent EF, duo anguli BEF, BFE, duobus DEF, DFE, ſinguli ſingulis æquales. Reliqua igitur reliquis æqualia (per El. I. 26.). Quare BE = ED. *Q. E. D.*

Jam vero puta arcum AC inæqualiter in E diviſum, et ſegmentorum AE, CE, majus eſſe AE, CE minus. (fig. 2.) Dico rectam BD inæqualiter in puncto E diviſam, ſegmentumque BE majus eſſe, DE minus. Jungantur enim ut prius FB, FE, FD, quarum FB, FD peripheriæ in punctis G, H, occurrant. Rectæ FB, FD, arcus AE, CE, bifariam dividunt. Arcus igitur GE, HE, arcuum AE, CE, ſemiſſes. Cum igitur arcus AE, arcu CE major ſit, arcus GE arcu HE major erit. Angulus igitur BFE angulo DFE major. Fiat angulus EFK angulo DFE æqualis. Quoniam angulus KFE angulo DFE æqualis eſt, nec non angulus rectus KEF, recto DEF æqualis; in triangulis EFK, EFD, quæ latus EF commune habent, anguli duo KFE, KEF, duobus DFE, DEF, ſinguli ſingulis æquales. Reliqua igitur reliquis æqualia. Latera igitur EK, ED, æqualia. Propter angulum vero EFK angulo EFB minorem, punctum K punctis, B, E, neceſſario interjacet. Recta igitur BE, recta KE major. Major itaque quam ED. *Q. E. D.*

THEOREMA II.

Linea recta quæ arcum circularem contingentibus duabus interceptum in puncto medio contingit, et contingentibus primo poſitis hinc inde occurrit, minima eſt omnium quæ, eundem arcum contingentes, contingentibus primo poſitis intercipiuntur.

ARCUM circularem BED, contingentibus duabus AB, CD, interceptum recta AC in puncto E contingat, et contingentibus primo poſitis AB, CD, in punctis A, C, occurrat. Punctum E arcûs BED medium eſto. Dico rectam AC omnium minimam, quæ, arcum BED contingentes, contingentibus AB, CD, intercipiuntur. Sumatur enim in arcu BED punctum quodlibet F, et ducatur recta GH quæ circulum in F contingat. Recta vero GH contingentibus AB, CD, in punctis G, H, occurrat. Dico rectam AC rectâ GH minorem. Si parallelæ ſint contingentes AB, CD (fig. 1.) res manifeſta eſt: parallelas enim AB, CD, recta GH oblique ſecat, recta autem AC normaliter. Rectæ vero AB, CD, non ſint parallelæ. (fig. 2. et 3.) Si recta AC non ſit minor quam GH, aut æqualis erit, aut major. Sit primo æqualis. Arcûs autem BD, inæqualiter in F diviſi, ſegmentum majus eſto BF. Rectæ igitur GH, inæqualiter in F diviſæ, ſegmentum GF majus erit (per 1. hujus). Rectæ BA, DC, productæ occurrent; occurſus eſto K: rectarum vero GH, AC, occurſus eſto L. Junctâ BD, per H ducatur HM rectis BD, AC, parallela: et per G ducatur GN rectæ DK parallela, quæ rectæ AC in N occurrat.

Rectæ GF, GB, quæ circulum in punctis B, F, contingentes in puncto G conveniunt, æquales ſunt. Pari ratione AE, AB, æquales. Recta igitur GF duabus AE, AG, ſimul ſumptis æqualis eſt. Rurſum rectæ HF, HD, quæ circulum in punctis F, D, contingentes in puncto H conveniunt, æquales ſunt. Pari ratione CD, CE, æquales. Recta igitur CD, vel CE, duabus FH, HC, ſimul ſumptis æqualis. Tres igitur GF, FH, HC, ſimul ſumptæ, tribus AG, AE, EC, ſimul ſumptis æquales; id eſt, duæ GH, HC, ſimul ſumptæ duabus AG, AC, ſimul ſumptis. Rectæ autem GH, AC, ex hypotheſi, æquales. Ablatis igitur GH, AC, æqualibus, relinquuntur HC, AG, æquales. Propter rectas autem AC, MH, BD, parallelas, et triangulum BKD iſoſceles, æquales ſunt CH, AM: quare AG, AM, æquales. Propter parallelas, autem AL, MH, rectæ GM, GH, in punctis A, L, ſimiliter diviſæ ſunt: bifariam autem GM in A: quare et GH bifariam in L: et propter GN, CD, parallelas, CN bifariam in L. Cum CL igitur ſemiſſis ſit rectæ CN, et CE ſemiſſis rectæ CA, erit EL rectæ AN ſemiſſis, ſive AN rectæ EL dupla. Rectâ autem GH æqualiter in L diviſâ, cum ejuſdem rectæ, inæqualiter in F diviſæ, ſegmentum GF majus eſt (per 1. hujus), recta GF, rectis HF et duplæ FL ſimul ſumptis æqualis eſt. Rectæ autem GF recta GB æqualis. Quare GB, ſive duæ GM, MB, ſimul ſumptæ, duabus, HF et duplæ FL, ſimul ſumptis, ſive duabus, HD et duplæ FL, ſimul ſumptis, æquales. Et ablatis hinc inde MB, HD, æqualibus, relinquetur GM æqualis duplæ FL, id eſt duplæ EL, id eſt, ex prius oſtenſis, rectæ AN. Propter æquales autem GA, AM, recta GM rectæ GA dupla eſt: et propter parallelas GN, KC, triangula AKC, AGN ſimilia: latera autem KA, KC æqualia: æqualia igitur GA, GN. Duæ igitur GA, GN, ſimul ſumptæ, duplæ GA æquales ſunt; id eſt, rectæ GM. Rectæ autem GM, AN oſtenſæ ſunt æquales. Duæ igitur GA, GN, ſimul ſumptæ, rectæ AN æquales ſunt: duo nempe trianguli latera ſimul ſumpta æqualia reliquo. Quod eſt abſurdum. Non ſunt igitur AC, GH, æquales. Dico neque majorem eſſe AC quam GH. Eſto enim major AC, ſiquidem eſſe poteſt. Duæ GH, HC ſimul ſumptæ duabus AG, AC ſimul ſumptis, ut prius, æquales ſunt. Cum igitur AC major ſit quam GH, erit AG minor quam HC. Æquales autem HC, AM, ut prius. Ergo AG minor erit quam AM; ac proinde, propter rectarum GM, GH, NC, ſimilem in punctis A, L, diviſionem, GL minor quam LH, et NL minor quam LC. Cum igitur CE ſemiſſis eſt rectæ CA, et CL major quam ſemiſſis rectæ CN, erit EL minor quam ſemiſſis rectæ AN: ſive AN duplâ EL major. Porro cum rectæ GH inæqualiter in L diviſæ, ſegmentum GL, ex oſtenſis, minus eſt, ejuſdem autem rectæ inæqualiter in F diviſæ ſegmentum GF majus, (per 1. hujus) duæ, FH et dupla FL, ſimul ſumptæ rectâ GF majores ſunt, ſive æquali GB, ſive duabus GM, MB ſimul ſumptis. Et æqualibus FH, MB hinc inde ablatis, relinquetur dupla FL rectâ GM major. Æquales autem LF, LE. Quare dupla EL rectâ GM major: et AN, quæ duplâ EL jam oſtenſa eſt major, rectâ GM multo major erit. Propter GA minorem vero quam AM, recta GM duplâ AG major. Æquales autem ut prius GA, GN. Dupla igitur GA, duabus GA, GN ſimul ſumptis æqualis eſt. Recta igitur GM, duabus GA, GN ſimul ſumptis major. Et proinde recta AN, quæ oſtenſa eſt major quam GM, duabus GA, GN, ſimul ſumptis multo major. Trianguli igitur AGN latus AN duobus reliquis ſimul ſumptis majus. Quod eſt abſurdum. Recta igitur AC rectâ GH major non eſt. Sed nec æqualis. Minor igitur. Simili ratione et aliâ omni minor, quæ arcum BD contingens contingentibus primo poſitis AB, CD intercepta eſt. Omnium igitur minima. *Q. E. D.*

THEOREMA III.

Polygonorum omnium, lateribus numero datis, datum circulum circumſcribentium, æquiangulum perimetro minimum eſt.

CIRCULUM ABCD circumſcriptum puta polygono, quot volueris laterum, FGHKE, quod omnium quæ, æquali laterum numero, circa eundem circulum circumſcribi poſſunt, perimetrum minimam habeat. Dico polygonum FGHKE æquiangulum eſſe. Nam ſi æquiangulum non ſit, neceſſe eſt ut duos aliquos angulos proximos inæquales habeat: nam ſi nullos proximos, omnino nullos; ſed æquiangulum erit. Sunto igitur inæquales proximi duo anguli GFE, KEF. Latera vero GF, KE, quæ cum intermedio FE angulos illos complexa ſunt, circulum in A, D punctis contingant: et latus intermedium FE eundem in L contingat. Circuli centrum eſto O. Jungantur OA, OL, OD. Anguli AFL, AOL ſimul ſumpti duobus rectis æquales ſunt, propter angulos ad A, L rectos. Similiter anguli DEL, DOL ſimul ſumpti duobus rectis æquales; propter angulos ad D et L rectos. Duo igitur AFL, AOL, ſimul ſumpti, duobus DEL, DOL ſimul ſumptis æquales. Inæqualibus igitur AFL, DEL hinc inde ablatis, relinquuntur AOL, DOL inæquales. Arcus igitur AL, LD inæquales. Bifariam igitur ſecetur arcus ALD in M puncto, quod neceſſario à puncto L diverſum erit. Per M ducatur recta quæ arcum AD contingat. Contingens per M contingentibus AF, DE in punctis N, P, occurrat. Recta NP rectâ FE minor erit (per præc). Quare et dupla rectæ NP duplâ rectæ FE minor. Sed propter NM=NA et PM=PD, tres rectæ AN, DP, PN, ſimul ſumptæ, duplæ rectæ NP æquales ſunt. Et propter FA=FL, et DE=EL, tres AF, DE, FE, ſimul ſumptæ, duplæ rectæ FE æquales ſunt. Tres igitur AN, DP, PN ſimul ſumptæ tribus AF, DE, FE, ſimul ſumptis minores. Polygonum autem NGHKP, circulum ABCD circumſcribit, et latera numero totidem habet ac polygonum FGHKE; quod omnium quæ æquali laterum numero, circulum ABCD circumſcribunt, perimetro, ex hypotheſi, minimum eſt. Perimeter igitur FGHKEF perimetro NGHKPN minor. Utrique auferatur pars communis AGHKD: relinquentur AF, DE, FE, reliquis AN, DP, PN, ſimul ſumptæ ſimul ſumptis, minores. Aſt majores jam oſtenſæ ſunt. Simul igitur majores et minores. Quod eſt abſurdum. Non ſunt igitur anguli GFE, KEF, inæquales, exiſtente perimetro FGHKEF minimâ. Similiter oſtendetur, nec alios duos quoſvis angulos proximos polygonum FGHKE inæquales habere.

Nullos igitur proximos inæquales habet. Omnino igitur nullos. Omnes igitur æquales. Æquiangulum igitur. *Q. E. D.*

THEOREMA IV.

Polygonorum omnium, lateribus numero datis, datum circulum circumſcribentium, æquiangulum areâ minimum eſt.

PATET ex præcedente, cum area æqualis eſt recangulo ſub ſemiperimetro et circuli inſcripti ſemidiametro.

THEOREMA V.

Polygonorum omnium, lateribus numero datis, dato circulo inſcriptorum, æquilaterum perimetro maximum eſt.

CIRCULO ABCD inſcriptum puta polygonum ABCDE, quot volueris laterum, quod omnium, quæ, æquali laterum numero, eidem circulo inſcribi poſſunt, perimetrum maximam habeat. Dico polygonum ABCDE æquilaterum. Non enim. Duo igitur proxima quædam latera inæqualia habet: nam ſi nulla proxima, omnino nulla; ſed æquilaterum erit; quod negaſti. Inæqualia ſunto latera proxima AB, AE. Propter rectas AB, AE inæquales, arcus AB, AE inæquales erunt. Biſariam igitur ſecetur arcus

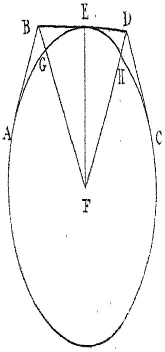
BAE in puncto G, quod à puncto A diverſum erit. Jungantur BG, EG. Circuli centrum eſto F: juncta GF peripheriæ iterum in H occurrat. Denique jungantur BE, HA. Arcubus GB, GE æqualibus, ſemicirculis GBH, GEH, ablatis, relinquuntur æquales BH, EH. Quare et anguli BGH, EGH, nec non BAH, EAH, æquales. Angulos igitur BGE, BAE, qui in eodem ſunt ſegmento circulari, rectæ GH, AH, bifariam dividunt. Duæ igitur GB, GE, ſimul ſumptæ ad rectam GH eandem proportionem habent, ac duæ BA, AE, ſimul ſumptæ, ad rectam AH. (*Vide* Demonſtrationem prop. 94. *Datorum* EUCLIDIS) In circulo autem ABCD, diameter GH, rectâ AH, major eſt. Duæ igitur GB, GE, ſimul ſumptæ, duabus AB, AE, ſimul ſumptis, majores (per xiv. 5. Elem.). Polygonum autem GBCDE circulo ABCD inſcriptum eſt, et latera numero totidem habet ac polygonum ABCDE; quod omnium quæ, æquali laterum numero, circulo ABCDE inſcribi poſſunt, perimetro, ex hypotheſi, maximum eſt. Perimeter igitur ABCDEA perimetro GBCDEG major. Utrique auferatur pars communis BCDE. Relinquuntur AB, AE, reliquis GB, GE, ſimul ſumptæ ſimul ſumptis, majores. Duæ autem GB, GE, duabus AB, AE, oſtenſæ ſunt majores. Simul igitur majores et minores. Quod eſt abſurdum. Non ſunt igitur latera AB, AE, inæqualia. Simili modo oſtendetur, de aliis quibuſlibet polygoni lateribus proximis, inæqualia non eſſe, exiſtente perimetro maximâ. Nulla igitur proxima inæqualia. Omnino igitur nulla. Omnia igitur æqualia. *Q. E. D.*

THEOREMA VI.

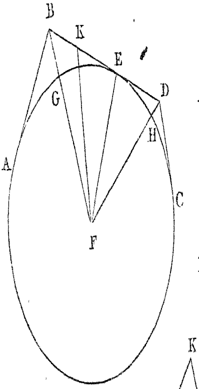
Polygonorum omnium, lateribus numero datis, dato circulo inſcriptorum, æquilaterum areâ maximum eſt.

Demonſtrationem vide apud THOMAM SIMPSON in libello ſuo de *Figuris Geometricis maximis et minimis.*

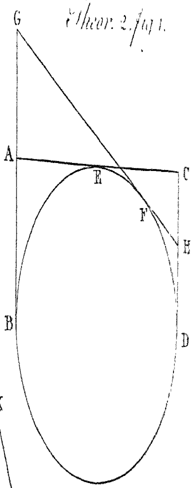
Theor 1 fig. 1.

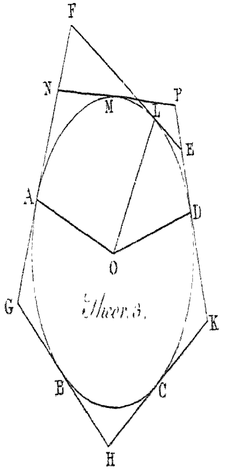


Theor 1. fig 2.

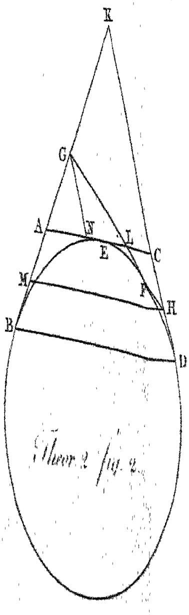


Theor. 2. fig 1.

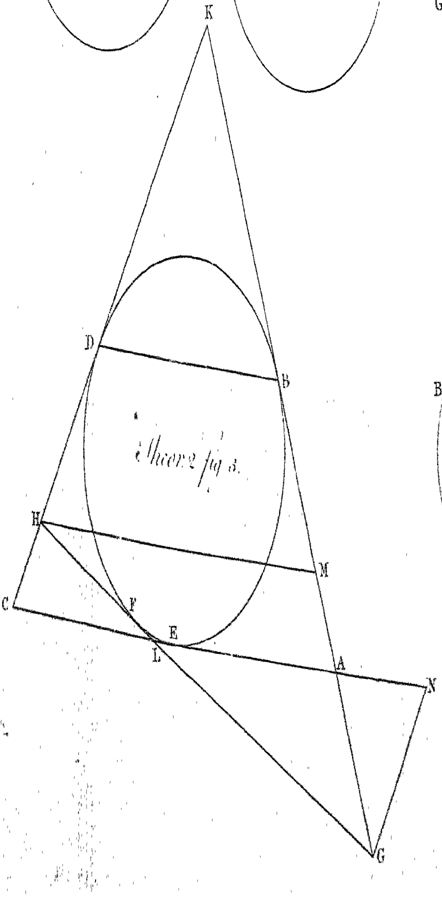




Theor. 3.

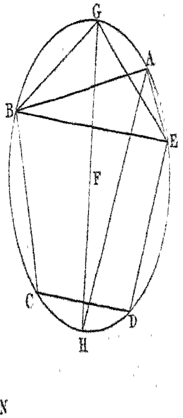


Theor 2 fig. 2.



Theor 2 fig 3.

Theor 5



XXX. *An Account of an extraordinary acephalous Birth. By* W. Cooper, *M. D. in a Letter to* William Hunter, *M. D. F. R. S.*

DEAR SIR,

Redde, April 27, 1775.

I TAKE the liberty to remit you an account of the delivery of a very curious acephalous monſter, accompanied with a ſhort deſcription of its anatomical ſtructure. If, after a peruſal of it, you ſhould apprehend it may be acceptable to the Royal Society, I beg that you will do me the honour to lay it before them.

Mrs. BRACKETT, of Clerkenwell Cloſe, aged twenty-three years, was, at the end of her firſt pregnancy, by a natural labour, delivered of a perfect female child, on Friday the 8th of October, 1773, at ſeven o'clock in the morning. The attending midwife, Mrs. AYRES, ſoon perceived by the abdominal tumour that there was another child. After waiting about three hours, a flooding came on; but without pain, or any advancement of the ſecond delivery. The *hæmorrhage* producing faintneſs, debility, and danger, the attendants and midwife were alarmed, and I was ſent for. When I came, I found her in the ſituation above deſcribed; and therefore thought it my duty to accompliſh the remaining part of the labour, as ſoon as I could, conſiſtently with the ſafety of the mother. Upon all occaſions, when the concomitant circumſtances render it neceſſary to turn a child *in utero*, it is of the utmoſt conſequence, to underſtand, as nearly as we can, its general ſituation, in order to deliver with the greater eaſe, ſafety, and expedition. And to an experienced *accoucheur*, if the breech, knees, or feet, do not immediately preſent themſelves, the head and face of the child will, in moſt caſes, be a ſufficient index to the poſition of the other parts of its body. This circumſtance ariſes from the *fœtus* commonly coiling itſelf up into an oblong, oval, ſnug, compact figure, with its knees towards its chin, in order to take up as little room as poſſible, by being adapted to the cavity of the *uterus*. In the preſent caſe, when the patient was placed in a proper ſituation, having introduced my hand as gently as poſſible through the *vagina*, *cervix uteri*, and inveloping membranes, and no part of the inferior extremities, or breech, preſenting itſelf, I examined carefully for the head of the child, as uſual, but without ſucceſs. This diſappointment ſomewhat embarraſſed me. But as the woman's ſituation was become very ſerious by the increaſing *uterine hæmorrhage*, I attempted without delay to get at the feet. I eaſily ſecured one of them; but though I made uſe of very little force in bringing it towards the *os externum*, the ſtructure was ſo very tender that the *tibia* began to give way at its ſuperior *epiphyſis*. On this account I was reduced to the diſagreeable neceſſity of again introducing my hand into the *uterus*; and as one leg had thus unexpectedly failed me, I thought it extremely futile to attempt any thing with the other. The moſt eligible reſource which I apprehended I had now left, was to fix a blunt hook upon one groin, and, when it was brought low enough, to aſſiſt gently at the other, with the two fore-fingers of my right hand. By theſe means I happily accompliſhed the delivery of the remaining *fœtus*, which proved to be a very ſingular kind of monſter. And as the late ingenious Mr. HEWSON injected its blood veſſels, and diſſected it, I am enabled to attempt a ſhort anatomical deſcription of it, for the ſatiſfaction of the curious in philoſophy and phyſiology.

This extraordinary animal production is of the ſize and appearance of a common twin child at its full time, excepting the particularities now to be pointed out. When firſt born it was very plump, but ſoft and flabby, and the bones remarkably ſmall and tender. It has neither head, neck, hands, or arms. In the place where the neck ſhould originate, is a little *mamilla*, ſomewhat larger than a woman's nipple, but quite ſoft. And on each ſide, in the place where the arm ſhould begin, there is a ſmall *papilla*, about the bigneſs, and very much like the extremity, of a common quill. The ſpine ſeems perfect, but ends abruptly at the upper *vertebræ colli*. Below the navel the parts are nearly intire, except the feet, where the toes are of an irregular form and ſize, and ſome of them united together. The external parts of generation, which indicate it a female, are alſo perfect.

Upon

Upon a careful inſpection internally, there is evidently no brain nor ſpinal marrow. A few nerves, however, are ſcattered about the *abdomen*; but their origin, for fear of deſtroying the preparation, is not traced. The *uterus* is perfect; but only one *ovarium* could be found. There is alſo the appearance of a bladder; but it is ſo contracted as to have no cavity. A large inteſtine ariſes from the *anus*; is a good deal convoluted when it gets above the brim of the *pelvis*, and ends in a blind pouch or *cul de ſac*, on the left ſide of the *abdomen*. This *viſcus* appears to be about ſix or ſeven inches in length, varies its ſize in different parts, gradually becomes ſmaller towards its ſuperior extremity, and ſeems fully diſtended with a colourleſs *mucus*(a). All above the navel is extremely defective. There is no heart, lungs, diaphragm, ſtomach, liver, kidnies, ſpleen, *pancreas*, nor ſmall inteſtines. However, there are three ſmall glands in the place of the *thymus*, whoſe ſubſtance, when examined with a microſcope, Mr. HEWSON remarked, exactly reſembled that of the *thymus* itſelf. And on each ſide of the *vena cava*, juſt under the navel, are two little glandular ſubſtances, which ſeem to be ſomewhat like *capſulæ renales*, only very ſmall to what are commonly found(b). There is a large artery running upon the ſpine, which may be called the *aorta*. As this approaches the upper extremity of the little animal, it is divided into ſmaller and ſmaller branches: and in its courſe it diſtributes lateral ones alſo to the contiguous parts of the trunk. Below the navel it ſends off two branches that conſtitute the umbilical arteries, one of which is conſiderably larger than the other. And then below theſe, two other branches deſcend to the inferior extremities. A large umbilical vein comes in at the navel, and is immediately divided into two conſiderable branches; one aſcending, the other deſcending. Each of theſe is again ſub-divided into ſmaller and ſmaller branches, which, as they paſs upwards and downwards, ſeem to correſpond with the different ramifications of the aſcending and deſcending *aorta*. The *funis umbilicalis* was only about two inches in length(c), and ſo very tender alſo, that it unavoidably ſeparated near the navel of the child during the delivery. Whether, therefore, there was any pulſation in this ſhort *funis* I am not able to determine. The *placenta* was not particularly examined.

(a) Does not this circumſtance almoſt amount to a proof, that the *meconium*, univerſally found in the bowels of new-born children, is nothing more than the *mucus* naturally ſecreted by the inteſtinal glands, mixed with bile, and perhaps a ſmall portion of the *pancreatic* juice? In the preſent inſtance, as there is no liver there could be no bile, and conſequently the *meconium*, if I may ſo call it, is colourleſs.

(b) Mr. HEWSON, ſome time before his death, ſeemed to be confirmed in the opinion, that whenever children are born with little or no brain, the *capſulæ renales* are always very much diminiſhed. This is certainly the caſe in one or two almoſt brainleſs children which I have by me, and whoſe *renal capſulæ* he examined, with a view of being further ſatiſfied upon this ſubject.

(c) An exactly ſimilar circumſtance to this I took particular notice of, in the delivery of another almoſt brainleſs monſter.

Here are evidently in this *fœtus* two diſtinct ſyſtems of veſſels, arteries and veins(d), that carried red blood(e). It is plain alſo, that the blood paſſed from the internal *iliac* arteries, through the *hypograſtrics* and *umbilicals* to the *placenta*, and was returned from it by the umbilical vein to the navel, and thence diſtributed in the manner before obſerved. But as there is no heart, nor any thing analagous to one, it becomes extremely difficult to aſcertain the powers by which the circulation was carried on through this curious phyſiological phænomenon. May we not however venture to advance a conjecture, that the periſtaltick, or living muſcular power of the arteries, was principally ſubſervient to this important end? Many examples are to be met with in the collections of the curious and learned in the different parts of Europe(f), which are ſomewhat ſimilar to that now related. When carefully examined, however, excepting a very few inſtances, they are generally found either eſſentially to differ, or elſe their ſtructure has not been, with any tolerable preciſion, explained. The preſent hiſtory affords alſo an exception to a frequent remark among authors, "That brainleſs children are always very briſk "before they are born (g);" for the mother has frequently told me, "That ſhe felt no motion at all within "her after the firſt birth; and that ſhe had not the leaſt "ſuſpicion of there being a ſecond child till it was deli"vered." This circumſtance may however, perhaps, be attributed to the *medulla ſpinalis* being totally deficient, as well as the *cerebrum* and *cerebellum*.

(d) Mr. HEWSON attempted to inject the whole blood veſſels by the umbilical vein as uſual. To his great ſurprize, no part of the injection returned by the umbilical arteries. He could not account for this ſingularity at that time: but as only a part of the veſſels were filled, he injected afreſh by one of the *hypogaſtric* arteries. Upon diſſection afterwards, this myſtery was unravelled by the heart's being totally abſent. It then appeared alſo, that by the firſt inſpection he had filled the venal ſyſtem, and by the latter the arterial.

(e) See a very curious caſe related by Monſ. WINSLOW, in the Memoires de L'Académie des Sciences for 1740, p. 586. and 604. Among other remarkable ſingularities in this little monſtrous abortion of ſix months, that excellent anatomiſt particularly takes notice, that there was no appearance of one drop of red blood in any of its veſſels, which were univerſally filled with a ſerous lympth; and that there were no veſtiges of any veins at all.

(f) F. LICETUS, de Monſtris, p. 300. et ſeq. PALFYN, Traité des Monſtres, p. 325. CHESELDEN's Anatomy, 5th ed. p. 379. Philoſophical Tranſactions, 1739-40, N° 456. p. 303. Ibid. 1767, p. 1. L'Académie des Sciences, Hiſt. 1720,

Phyſiologiſts and philoſophers have ſpent a great deal of time in attempting to inveſtigate the cauſes of theſe extraordinary phænomena. With this view many opinions have been ſtarted; but moſt, if not all of them, as far as I am able to judge, being built upon the tottering baſis of conjecture only, afford, upon an attentive inſpection, but little ſatisfaction to a diſpaſſionate enquirer after truth. The particular hypotheſis, which has been almoſt univerſally adopted, is, that monſtroſity and marks in children depend upon the imagination and

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1720, p. 13. Ibid. Mem. 1720, p. 8. Ibid. 1740, p. 586. and p. 592. Miſcellania Curioſa Ephemeridum Germanicarum Ann. XIX. p. 258. Acta Eruditorum Lipſiæ, Ann. 1724, p. 501.

(g) Philoſophical Tranſactions, 1674, N° XCIX. p. 6157. Ibid. 1767, p. 18.

longing of the mother. Such pernicious a principle as this ought to have very rational evidence, and the moſt ſtriking facts to ſupport it. But is it not directly the contrary? Indeed a great many ridiculous ſtories have been related to the world(b), which, however, upon a little reflection either obviate themſelves, or elſe are contradicted by thoſe facts that occur. May we not exemplify this obſervation by the caſe of twins now related? One of the children was perfect, and is ſtill living; the other proves to be remarkably defective. Does not the queſtion naturally ariſe here, how could one child be affected by the diſturbed imagination of the mother and not the other? But the mother, upon repeated examination, recollects no fright in particular while ſhe was pregnant. Neither, if ſhe did, would it all invalidate the force of our argument upon this ſubject; for ſhe could not poſſibly ſee any child without a head: and more eſpecially, becauſe other parts, as the *viſcera* and *medulla ſpinalis*, were equally defective, which are entirely out of the reach of the eye or imagination of the mother to form any idea about them. To elucidate this point ſtill further, can any candid perſon poſſibly ſuppoſe, that the caſual agitation of mind of a pregnant woman, ſhould either produce or deſtroy a whole ſyſtem of blood-veſſels, nerves, and fibres, which are indiſpenſable conſtituents of almoſt every part of the body? And may we not adduce one proof more, in ſupport of our argument, from what happens to animals and vegetables? Among theſe alſo, ſuch extraordinary deviations from the general courſe of nature are by no means uncommon: yet the former are poſſeſſed of a much leſs ſhare of imagination than is generally allotted to the human ſpecies; and the latter have none at all. Reaſoning in the ſame manner upon ſeveral occaſions of this kind in which I have been concerned, my concluſions have always been ſimilar; *viz.* that the uſually aſſigned cauſe of the mother's imagination is by no means equal to the manifold effects produced. And on the other hand, this injurious doctrine is pregnant with continual miſchief to ſociety. It frequently makes women very unhappy. And the fear of mutilating or marking their infants often affects them ſo much, that they at laſt miſcarry. Having therefore indubitable facts to go upon, and the cauſe of humanity ſo powerfully coinciding with the truth, is it not right to affirm and maintain with confidence, that neither the longing nor frighted imagination of the mother appears to have any power at all to imprint marks or monſtroſity upon children? That this is a very weak ſuppoſition, entirely void of foundation, directly contrary to all philoſophy and experience, and has nothing to ſupport it but a vulgar opinion, tranſmitted to us from the ages of anatomical ignorance? And is it not more reaſonable to conclude with you, SIR, in your extenſively uſeful lectures, that whatever be the defect or deformity in a monſtrous birth, it can never be occaſioned by accidents of any kind during pregnancy; but probably has its exiſtence always originating, *cauſâ adhuc incognitâ*, in the firſt ſtamina of the embryo (i)?

(b) MAURICEAU, p 53. Obſ. 64. Ibid. p. 63. Obſ. 118. SMELLIE's Midwifry, vol. III. p. 402. Philoſophical Tranſactions, 1684, N° 160. p. 599. Ibid. 1739-40, N° 456. p. 303. and 306.

Thus, SIR, I have faithfully related the particulars of this ſingular phænomenon among the human ſpecies, which, to a demonſtration, confirms your opinion, that the nouriſhment of the *fœtus in utero* is principally by means of the *funis umbilicalis*. M. MERRY obſerves, that defective monſters are more inſtructive than others that have redundancies (k). If this be true, here is ſtill an ample field for ſpeculation, notwithſtanding the few very obvious remarks which I have already ventured to make. In conformity to the general language of authors, I have in this eſſay occaſionally adopted the uſe of the term *monſter*. There is, however, ſomething in that word extremely repugnant to our common feelings, and very apt to leave a terrifying impreſſion upon the mind. Why may not the Author of Being ſometimes produce variations in the human ſpecies, as well as in the animal and vegetable kingdoms (l), and equally exempt too from ſuch frightful appellations? Would it not, therefore, be more eligible in the preſent inſtance, and every ſimilar one, to explode the common term, and call it ſimply a *luſus naturæ*; or with PLINY to ſay, "*Hoc nobis miraculum, ſibi ludibrium, ingenioſa finxit natura.*"

(i) The great baron HALLER is of opinion alſo, that this is evidently the caſe in that ſpecies of monſters to which parts are added. Vide Opera Minora HALLERI, tom. III. p. 148.

(k) L'Académie des Sciences, Hiſt. 1720, p. 13.

(l) See F. LICETUS, J. PALFYN des Monſtres, &c. in which are many inſtances of each kind.

The peculiar ſhare of your friendſhip, with which you are pleaſed to honour me, makes me flatter myſelf that you will give me credit for the truth of any facts advanced in the courſe of the detail, that are not irrefragably evidenced by a recourſe to the preparation in its preſent ſtate.

I am, &c.

Norfolk-ſtreet,
June 6, 1774.

XXXI. *Obſervations on the State of Population in* Mancheſter, *and other adjacent Places, concluded. By* Thomas Percival, *M. D. F. R. S. and S. A. Communicated by the Rev. Dr.* Price, *F. R. S.*

Redde, Feb. 2, 1775.

A VERY accurate ſurvey was completed laſt year, of the towns of Mancheſter and Salford, with their reſpective townſhips. This ſpring an enumeration, equally exact and comprehenſive, has been made of the whole pariſh of Mancheſter, which contains thirty-one townſhips (excluſive of the two above mentioned) in the compaſs of leſs than ſixty ſquare miles. The reader is here preſented with the particulars of this enumeration.

Tenanted houſes,	2371	Under 15,	5545
Families,	2525	Above 50,	1762
Inhabitants,	13786	Above 60,	470
Males,	6942	Above 70,	261
Females,	6844	Above 80,	87
Married,	4319	Male lodgers,	68
Widowers,	23	Female ditto,	51
Widows,	315	Empty houſes,	41

The number of perſons to a houſe in Mancheſter is therefore nearly $5\frac{4}{5}$; of individuals to a family, about $5\frac{1}{2}$;

and $\frac{1}{5}$th of the inhabitants have attained the age of 50. It is unneceſſary to point out the difference in the proportions between the town and the adjacent country, as it will appear ſufficiently obvious by comparing this account with that of Mancheſter. The whole number of the inhabitants in the town, townſhip, and pariſh of Mancheſter amounts to 42937.

At the cloſe of 1772, an account was collected from every country chapel, both epiſcopal and diſſenting, in the pariſh, of the baptiſms and burials of that year. The former were found to amount to 401, the latter to 246; and there is a preſumption that this is nearly the annual proportion of deaths in the pariſh of Mancheſter, excluſive of the town and townſhip. For the number of burials in the whole pariſh was, in the ſame year exactly 1200; and it has been ſhewn, that the deaths in the town of Mancheſter are, one year with another, 958. This ſum being ſubtracted from 1200, leaves a remainder (242) for the country, very nearly equal to 246. And if 13786, the number of people in the pariſh, be divided by 246, it will appear, that only 1 in 56 of the inhabitants dies annually, whilſt the yearly mortality in Mancheſter is as 1 to 28. Such a ſtriking diſparity in the healthineſs of a large town, and of the country which ſurrounds it, granting it to be leſs than has been ſuppoſed, will ſcarcely be credited by thoſe who have paid no attention to enquiries of this nature. And it muſt afford matter of aſtoniſhment even to the phyſician and philoſopher, when he reflects that the inhabitants of both live in the ſame climate, carry on the ſame manufactures, and are chiefly ſupplied with proviſions from the ſame market. But his ſurprize will give place to concern and regret, when he obſerves the havoc produced in every large town by luxury, irregularity, and intemperance(a); the numbers that fall annual victims to the contagious diſtempers, which never ceaſe to prevail; and the pernicious influence of confinement, uncleanlineſs, and foul air on the duration of life(b).

> “ It is not air, but floats a nauſeous maſs
> “ Of all obſcene, corrupt, offenſive things.”
>
> ARMSTRONG on Health, book I.

Great towns are in a peculiar degree fatal to children. Half of all that are born in London die under three, and in Mancheſter under five years of age; whereas at Royton, a manufacturing townſhip in the neighbourhood of Mancheſter, the number of children dying under the age of three years, is to the number of children born only as one to ſeven; and at Eaſtham, a pariſh in

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(a) There are at this time in Mancheſter no leſs than 193 licenſed houſes for retailing ſpirituous and other liquors, and 64 in the other townſhips of the pariſh. At Birmingham, the number of public houſes is ſtill greater than at Mancheſter. A very ingenious friend of mine in that place has computed, that the quantity of malt conſumed there in the public houſes, requires for its growth, a compaſs of land which would be ſufficient for the ſupport of 20,000 men.

(b) The rev. Dr. Tucker, dean of Glouceſter, informs me, “ That were it “ not for the daily arrival of recruits from the country, his pariſh (St. Stephen's “ in Briſtol), and indeed Briſtol in general, would be left in a century without “ an inhabitant, unleſs the people would betake themſelves to better courſes.”

Cheſhire, inhabited by farmers, the proportion is conſiderably leſs (c).

It is a common but injurious practice in manufacturing countries, to confine children, before they have attained a ſufficient degree of ſtrength, to ſedentary employments, in places where they breathe a putrid air, and are debarred the free uſe of their limbs. The effect of this confinement, ſays an able writer, is either to cut them off early in life, or to render their conſtitutions feeble and ſickly; but the love of money ſtifles the feelings of humanity, and even makes men blind to the very intereſt they ſo anxiouſly purſue. The ſame principle of ſound policy which induces them to ſpare their horſes and cattle, till they arrive at a due ſize and vigour, ſhould determine them to grant a proportionable reſpite to their children (d). And this obſervation may, perhaps, be extended to the untimely culture of the mind. For too early an application to ſtudy impairs the faculties, injures the conſtitution, and hurts the temper by frequent contradiction. Almoſt as ſoon as a boy has acquired the powers of ſpeech, he is ſhut up many hours every day in a noiſome ſchool, ſecluded from the benefit of exerciſe and the refreſhment of the open air, and tied down to the ſevere drudgery of learning what ſerves only, at ſuch a period of life, to over-charge his memory, and to deſtroy his native cheerfulneſs of diſpoſition. Thus the age of gaiety (to uſe the words of the elegant writer before referred to) is ſpent in the midſt of tears, puniſhments, and ſlavery; and this to anſwer no other end, but to make a child a man, ſome years before nature intended he ſhould be one.

(c) See my former paper on Population. Phil. Tranſ. vol. LXIV. part I. page 62.

(d) See Dr. GREGORY's Comparative View of the State and Faculties of Man.

The reverend Mr. HARRISON, of Chapel in le Frith, has made a ſurvey, at my requeſt, of the inhabitants of Chinley, Brownſide, and Bugſworth, three hamlets contiguous to each other, in the pariſh of Gloſſop and Peak of Derbyſhire. They are four ſtatute miles in length, and three in breadth; and contain 301 males, 310 females; 200 married perſons, 15 widowers, 18 widows; 234 perſons under fifteen years of age, 121 above 50, and 9 who have attained the age of 80. This enumeration was finiſhed in September 1773.

I have been furniſhed by the reverend Mr. ASHETON, rector of Middleton, near Mancheſter, with an account of the births, deaths, and marriages, in his pariſh, during ten correſponding years of the laſt and the preſent century. From 1663 to 1672 incluſive, the deaths were males 180, females 187; the births, males 200, females 188; the marriages 121. The births, therefore, during ten years, only exceeded the deaths in number 21. And the average number of births to each marriage was as $3\frac{1}{5}$ to 1. From 1763 to 1772 incluſive, the deaths were 499 males, 494 females; the chriſtenings, 802 males, 768 females; the marriages 330. The baptiſms, therefore, during this period, exceeded the deaths 577;

that is, near 58 annually. And if no allowance be made for illegitimate births (which, I believe, in this pariſh are not numerous, and can no where be ſuppoſed to equal one fourth of all that are born), each marriage has produced $4\frac{3}{4}$ children. It is curious to obſerve the change in the proportion of births to the deaths, and alſo to the marriages, which has taken place at Middleton (and I have received ſimilar accounts of other places), during the courſe of the laſt century. The former may be explained by the greater encouragement to matrimony from the increaſe of trade; the latter is of more difficult ſolution; though it is probable, that the warmer cloathing and better fare, which the poor now enjoy, may have contributed to it. Luxury, when carried to ſuch a degree as to enervate the conſtitution, is unfavourable to population; but plenty of nutritive diet may well be regarded as a ſource of fruitfulneſs. The lower claſs of people in this county formerly lived upon the coarſeſt food. Wheat, an hundred years ago, was almoſt unknown to them; and ſo lately has it been cultivated in Lancaſhire, that it has ſcarcely yet acquired the name of corn, which in general is applied only to barley, oats, and rye. Potatoes alſo are much improved by the preſent judicious method of growing and propagating them; and they now conſtitute a moſt wholeſome and nouriſhing part of our diet. Perhaps likewiſe the general uſe of pepper and of other ſpices may increaſe the fertility of mankind. But I ſhall ſuſpend my conjectures for the preſent. A variety of cauſes may counteract the operation of thoſe which I have enumerated, and a conſiderable number of facts muſt be adduced, to aſcertain, whether the proportion of births to marriages be generally increaſed in countries advanced from poverty to wealth, by the introduction of trade, or the improvements of agriculture. The inſtance of Middleton and of one or two places more which firſt occurred to me, and ſuggeſted the preceding obſervations, is oppoſed by others which have lately fallen under my notice. And I cannot cloſe this ſubject better than by giving a view of the facts which I have collected on both ſides the queſtion.

A TABLE ſhewing the proportion of births to marriages in different places, and and at different periods of time.

Year.		Marriages.	Chriſtenings.	Births to a Marriage.
		Middleton.		
From 1663 to	1672	121	388	$3\frac{1}{5}$+
1763	1772	330	1570	$4\frac{3}{4}$
		Warrington.		
From 1702 to	1722	131	385	2.9+
1752	1772	1549	5034	$3\frac{1}{4}$
		Pentraeth Pariſh, Angleſey (e).		
From 1740 to	1747	32	100	$3\frac{1}{8}$
1764	1771	33	149	$4\frac{1}{2}$

(e) Vide Philoſophical Tranſactions, vol. LXIII.

Llanddyfnan

Llanddyfnan Pariſh, Angleſey (f).

Year.		Marriages.	Chriſtenings.	Births to a Marriage.
From 1750 to	1757	28	111	3.9+
1764	1771	32	154	$4\frac{4}{5}+$
1547	1554	8	36	$4\frac{1}{2}$
1620	1627	20	44	$2\frac{1}{5}$

Liverpool.

From 1700 to	1710	500	2127	$4\frac{1}{4}$
1762	1771	4812	10010	$2\frac{1}{12}-$

Bowden.

From 1653 to	1662	136	573	$4\frac{1}{5}+$
1763	1772	369	1300	$3\frac{1}{2}+$

I have lately received, from the reverend Mr. Archdeacon Blackburn, rector of Richmond, in Yorkſhire, the following account of his pariſh. From the year 1764 to 1773 incluſive, 452 males and 376 females have been baptized; 299 males and 341 females have been buried. The marriages, during this period, have amounted to 200. In Richmond there are about 600 houſes; but the Eaſter book enumerates only 450 families, and Mr. BLACKBURN computes the number of inhabitants to be 2300. "We have no diſtempers, he ſays, "that can be called endemial; and when fevers prevail "in the neighbourhood, few are affected by them in "this town. If any perſon brings an ague to Richmond, he is generally freed from it in a few days;

(f) Vide Philoſophical Tranſactions, vol. LXIII.

"though the village of Gilling, about a mile and a half "diſtant, which ſtands low, and has a large pool of ſtagnant water adjoining to it, is viſited with this complaint every ſpring and autumn. The air of Richmond ſeems to be peculiarly unfavourable to conſumptive diſorders. Many ſtrangers come hither from "different parts in the firſt ſtage of the *phthiſis pulmonalis*; but after thirty-five years experience, I may "truly ſay, that not one has recovered, although the utmoſt care and attention have been paid to their reſpective "caſes. The natives and conſtant reſidents are not ſubject to diſtempers of the lungs, except when brought "on by intemperance. But rheumatic complaints are "very general, eſpecially amongſt the ſenior part of the "inhabitants. In ſmall corporation towns, like Richmond, numbers are taken off by exceſſive drinking; "but the people here, who live temperately, ſeldom die "earlier than in their eightieth year."

Happening to paſs through Sutton Coldfield, in Warwickſhire, laſt ſummer, I was very much ſtruck with the beauty and apparent healthineſs of its ſituation, and was deſirous of knowing the duration of life which the inhabitants of it enjoy. The rector of the pariſh has, with great politeneſs and good-nature, gratified my curioſity as far as he is able, by furniſhing me with an extract from the church regiſter, and by referring me to the XXXIId volume of the Gentleman's Magazine for the following authentic account of the place, drawn up, I ſuppoſe, by himſelf.

"Sutton

“ Sutton Coldfield is almoſt full South of Litchfield, at the diſtance of about eight meaſured miles, by which it undoubtedly got its name of Sutton, a contraction of South Town. A remarkable bleak and barren common, which lies directly Weſt of it, juſt out of the bounds of the pariſh, might probably give it the additional denomination of Coldfield. The air upon that heath, as travellers have declared, is as keen and cold as that upon the Highlands of Scotland. The pariſh is nearly oval in its figure, the longeſt diameter ſeven miles, and the breadth four. The face of it is agreeably diverſified with gently riſing hills, and vallies of tolerably fruitful meadows. It is bounded on the North by Shenſton, on the Weſt by Barr, on the South by Curdworth and Aſton near Birmingham, and on the Eaſt by Middleton. It contains four hamlets; *videlicet*, Mancy, Hill, Little Sutton, and Warmley. In the year 1630, there were 298 houſes in the pariſh; in 1698 there were 310; in 1721 the number was increaſed to 360; which is nearly about the number at preſent. I compute the inhabitants at 1800. The regiſter begins in the year 1603. The number of chriſtenings for the firſt twenty years of the regiſter was 645; the burials during the ſame period were 501. The number of chriſtenings for the laſt twenty years (ending at Chriſtmas 1761) was 747; the burials 694(g).” If the number of inhabitants in this town be rightly computed, the yearly mortality amongſt them is not ſo much as 1 to 51; and every houſe contains at a medium five perſons.

(g) Gent. Magazine for September 1762, p. 421.

It appears by the obſervations lately communicated to me by the rev. Dr. TUCKER, that the number of females baptized at the pariſh church of St. Stephen's, in Briſtol, from 1754 to 1774, has exceeded the number of males baptized during the ſame period of time; and that the like remark has been made in ſome other pariſhes of that city. From theſe facts the learned dean concludes, that Mr. DERHAM's calculation, which ſuppoſes the proportion of male to female births to be as 13 to 12, may poſſibly be erroneous; and he expreſſes his earneſt wiſhes, that farther enquiry may be made into a ſubject of ſo much importance. The following table will ſhew the reſult of the few obſervations which I have collected.

A comparative view of the number of males and females baptized in different places.

Places.	Males.	Females.
Diſhley, 11 years,	149	145
St. Stephen's pariſh, Briſtol, 20 years,	591	607
Taxal, 15 years,	204	230
Richmond, 10 years,	452	376
Middleton, 10 years,	200	188
Bowden, 10 years,	663	639
Middlewich, 5 years,	229	242
Chapel in le Frith, 10 years,	451	332
Warrington, 1 year,	175	181
Collegiate Church, Mancheſter, 7 years,	3215	3024
Royton, 10 years,	134	120
Cheſter, 2 years,	408	415
Total,	6871	6499

This table ſufficiently confirms the calculation of Mr. DERHAM, with reſpect to the greater proportion of male than of female births; but the ſucceeding table evinces, that the number of females living conſiderably exceeds the number of males in a variety of places, and that the widows are almoſt double the number of widowers.

A comparative view of the number of males and females in different places.

Places,	Males,	Females.
Mancheſter,	10548	11933
Salford,	2248	2517
Townſhips of ditto,	947	958
Pariſh of Mancheſter,	6942	6844
Bolton,	2159	2392
Little Bolton,	361	410
Monton,	196	190
Hale,	140	136
Horwich,	149	156
Darwen,	900	950
Cockey,	320	391
Chawbent,	554	606
Ackworth,	340	388
Eaſtham,	451	461
Chinley,	181	168
Brownſide,	40	47
Bugſworth,	80	95
Total,	26556	28642

A comparative view of the number of widowers and widows in different places.

Places.	Widowers.	Widows.
Mancheſter,	432	1064
Salford,	89	149
Townſhips of ditto,	21	42
Pariſh of ditto,	232	315
Monton,	14	13
Hale,	8	12
Horwich,	9	8
Darwen,	30	48
Cockey,	10	27
Chowbent,	26	43
Chinley,	27	31
Brownſide and Bugſworth,	15	18
Total,	913	1770

The reader will perceive, from theſe tables, that the proportion of males to females baptized, is nearly as 12 to $11\frac{1}{3}$, or 19 to 18; but that the number of females living is to the number of males as about 11 to $10\frac{1}{5}$, or more exactly as 14 to 13; and that the widows are almoſt double the number of widowers.

XXXII. *An Account of the Effects of Lightning on a Houſe, which was furniſhed with a pointed Conductor, at* Tenterden, *in* Kent. *In Two Letters from* Richard Haffenden, *Eſquire, the Proprietor of the Houſe, to Mr.* Henley. *To which are added ſome Remarks by Mr.* Henley.

Redde, May 4, 1775.

HAVING been informed by my friend Mr. CURTEIS, that the houſe of RICHARD HAFFENDEN, eſquire, at Tenterden, in Kent, had been damaged by a ſtroke of lightning, notwithſtanding it had been furniſhed with a metallic conductor to prevent thoſe accidents: I wrote to Mr. HAFFENDEN, requeſting the favour of the particulars, and eſpecially an anſwer to the following queſtions.

1. The length and breadth of the houſe? 2. The height of the chimney, to which the conductor was affixed from the ground, and how far it might riſe above the ridge or roof of the houſe? 3. To what height the conductor was carried up above the top of the chimney? 4. Whether it was terminated by a ſharp point, a blunt end, or a ball? 5. Whether it was in regular contact from the top to the bottom? 6. What was its diameter or breadth? 7. Was it carried down into the moiſt earth, or water? 8. What is the nature of the ſoil it enered? 9. Did any other chimney, or ſtack of

chimnies, ſuſtain a ſtroke, beſides that which carried the conductor: and what was the diſtance of each of the ſtricken chimnies from the conductor? and was there any communication of lead on the ridge or roof of the houſe, that might connect ſuch chimney, or part of the houſe in which a ſtroke was received, with the conductor? 10. What was the damage the houſe ſuſtained; and how was the electricity conveyed to the earth? 11. Doth your houſe ſtand ſingle? 12. Is it ſituated on a plain, or on an eminence? 13. Had it rained before the ſtroke; and was that part of the houſe on which the lightning fell, conſiderably wetted at the time? 14. Did the wind blow toward that part of the houſe? 15. When did the accident happen?

LETTER I.

SIR,

I RECEIVED yours of the 31ſt *ult.*; in anſwer to which, pleaſe to obſerve the following ſketch of the top of the houſe (*vide* TAB. VII. fig. B.). *a*, *b*, *c*, *d*, are four chimnies, about thirty-eight feet high, and two feet above the ridge of the houſe. *f* is the conductor, elevated about five feet above the top of the chimney: it is made of iron, about half an inch diameter, tapering to a point, gilt. After being fixed to the chimney, eight or ten feet, it turns under the roof to the leaden pipe (*g*), which carries the water from the gutters E, E, E, and goes down on the outſide of the houſe, till it reaches within four feet of the earth. In the lower end of this pipe is put one end of a ſquare rod of iron, about three quarters of an inch in breadth; the other end of the iron-rod reſts on the ground, at the diſtance of ſix feet from the foundation. The ſoil is light mould. The houſe ſtands ſingle, on the Weſt ſide of a hill; the front toward the hill. On the top of the hill, at about forty rods diſtance, is a wind-mill, whoſe bottom is nearly as high as the top of the houſe. About two of the clock in the morning of June 17, 1774, after thundering for ſix or ſeven hours at a diſtance, and coming ſlowly on, there being little or no wind, there came three amazing ſtrokes. It rained extremely at the time; but ceaſed immediately at the laſt ſtroke, which ſtruck the top of the chimney (*b*) the furtheſt from the conductor, and broke it down to the lead (*b*), where it divided; ſome up and down the rafters (*i*, *i*,) breaking and ſplitting both tiles and rafters into thouſands of pieces, and throwing ſome of them to a great diſtance, till it reached the gutter (E), which was full of water. Another part, or diviſion of the exploſion, broke the mortar down to the lead on the cornice (*k*, *k*, *k*,); which lead goes round the front of the houſe. Through this lead the lightning paſſed quietly, till it reached the chimney (*c*), and broke the mortar on that chimney, till it reached the lead adjoining to it; through which lead it paſſed quietly, and then broke the tiles to the lead on the chimney (*d*); and from thence it paſſed to the gutter or leaden pipe; and there, uniting with the other diviſion of the exploſion, it paſſed down the leaden pipe to the iron bar at the bottom; where, by an exploſion, it burſt a hole in the pipe, in paſſing from one metal to the other; and then went down the iron rod to the ground, flowing with water. It ſeems to me, according to the ſituation and direction of the cloud, being nearer to the gutters than the conductor, that *they acted as conductors*; and that the conductor itſelf was not in the leaſt affected by the ſtroke; and that had there been conductors on the chimnies (*a* and *b*) they would have conveyed the electric fluid to the ground, without any damage to the houſe: or had there not been a continuation of metal from the gutters, &c. to the earth, it would have damaged the houſe all the way down. There was another diviſion of the exploſion, which, I imagine, came down the chimney, and ſtruck to a bell-wire in the paſſage, and deſtroyed the wire to its end, which happened to be juſt within-ſide the wall, where the water-pipe was without; and there it made a ſmall hole, in the joint of the bricks of an eighteen-inch wall, to the pipe; and then paſſed to the earth with the reſt. I think this accident is no proof whatever of the preference of balls or points as terminations of conductors. Thus, SIR, I have endeavoured to anſwer every one of your queries; which, if ſatisfactory, will give pleaſure to, &c.

N. B. The pricked line in the figure ſhews where the lightning paſſed quietly, having a ſufficient ſubſtance of metal to paſs through. The *curved line* ſhews where it broke every thing. The diagonal lines are all lead.

LETTER II.

SIR,

I HAVE received your two laſt; and can aſſure you, it is with great pleaſure I anſwer any queſtion you ſhall be pleaſed to aſk. In anſwer to your firſt and ſecond queries; *videlicet*, What is the breadth of the houſe? and what is the diſtance between the extreme part of the chimney (*b*) and the conductor? The drawing I ſent you is a ſcale of one-tenth of an inch to a foot, or nearly ſo (a). 3d. The iron rod in the pipe is only a large *ruſty ſpit* put in, a few inches, *pro tempore*. The place where the leaden pipe is burſten, is even with the point of the ſpit; very little melted, but broke open with the exploſion. The pipe is bent outward, about a foot at the lower end, to carry the water from the houſe; the hole in which, and the point of the ſpit, are near the middle of that projecting part of the pipe. No viſible alteration appeared on the ruſty ſpit by the lightning having paſſed through it. The lower end of it lays *on*, not *in* the earth. I am, &c.

(a) Then, the breadth of the houſe is about twenty-nine feet, and the diſtance between the fartheſt corner of the chimney (*b*) and the conductor, about forty-nine or fifty feet. W. HENLEY.

THESE letters containing a ſatisfactory anſwer to the queſtions which precede them, the following remarks naturally preſent themſelves. 1ſt, A ſharp pointed conductor did not, in this inſtance, invite or draw down upon itſelf a ſtroke of lightning. 2dly, Such a conductor, elevated five feet above the top of one of the chimnies, to an houſe of this dimenſion, may not perhaps be ſufficient, by its ſilent attractive force, to protect the whole of ſuch a building from a ſtroke; eſpecially when a chimney, *a blunt body*, wetted with the rain, ſtanding at fifty feet diſtance from the conductor, and being within five feet of its height, is in actual contact with ſo large, though irregular, a communication of metal, leading from the chimney directly to the conductor; though, in this inſtance, it ſhould be remarked, that the conductor itſelf *was not in contact* throughout; and it is, for that reaſon, a very exceptionable caſe. 3dly, Two ſuch conductors; one, for inſtance, on the chimney (*a*), where this was placed; and the other, on the chimney (*b*), which was ſtricken, with a communication of lead between them, would probably have protected the houſe: but a conductor on each chimney would certainly have ſecured the whole building effectually (b). 4thly, As the three branches or diviſions of the lightning all concentrated upon an iron bar, *three quarters of an inch ſquare*, and produced no ſign of heat in it, an iron bar of that ſize ſeems to be fully ſufficient for the purpoſe. There ap-

(b) This method, I am informed, Mr. HAFFENDEN hath lately adopted.

pears, however, to have been two defects in Mr. HAFFENDEN's conductor: 1. The leaden pipe and the iron bar at the bottom were not in contact. 2. The iron bar, or a thick plate of lead, ſhould have been continued down into the moiſt earth or water; and had not the earth, as Mr. HAFFENDEN obſerves, flowed with water, at the time of the accident, the want of this precaution might, perhaps, have been attended with ſome damage to the foundation. I have been thus particular in my inquiries into, and remarks upon, this fact, as I think it of great importance to thoſe who erect conductors, to be informed of every defect in the conſtruction, and of the dangerous conſequences thence ariſing.

P. S. In Mr. HAFFENDEN's ſecond letter, he obſerves, that the bell-wire, mentioned in his firſt letter, was braſs; and that ſo much of it as went through the paſſage was painted: and the painted part, he ſays, was not deſtroyed; but the paint was looſened on the wire, without being broken off, like the looſe rind of a tree; which reſembles the effect of the artificial electricity, in an experiment of Mr. KINNERSLEY's, where a wire was, by a great exploſion, both leſſened in diameter, and extended in length. The other part of the wire, which was not painted, except a ſhort piece at the end, ſomewhat larger and of iron, was entirely melted. Query, If the wire before ſpoken of had paſſed through a ſtone, particularly a wet one, incloſing it firmly, would not that ſtone have been ſhivered to pieces?

XXXIII. *Of the Torpidity of* Swallows *and* Martins. *By* James Corniſh, *Surgeon, at* Totneſs, Devonſhire, *in ſundry Letters to the Honourable* Daines Barrington, *F. R. S. and Dr.* Maty, *Sec. R. S.*

LETTER I.

TO DR. MATY.

SIR, Totneſs, Feb. 3, 1775.

Redde, May 25, 1775.

AS it has long been a *deſideratum* among the naturaliſts to decide, with certainty, whether ſwallows and martins remain in a torpid ſtate during the winter, or are birds of paſſage; I ſhall make no apology for troubling you with this letter, as it determines one part of the queſtion, as I imagine, beyond doubt. In the beginning of November, being fiſhing on the banks of the river Dart, which runs at the bottom of a very ſteep hill, from the ſide of which project ſeveral large rocks, overgrown with ivy and thicket; I was at once ſurprized with the ſight of a great number of martins. Now the ſeaſon of the year being ſo advanced, I deſiſted from my amuſement, that I might the more carefully obſerve the birds, which, I concluded, had been brought out of their winter quarters by the fineneſs of the afternoon, it being remarkably pleaſant and warm for the time of the year; the Sun at that time darting its rays directly againſt the rocks, juſt oppoſite to which I had fixed my ſtation. They continued to flit to and fro for near half an hour, keeping very near together, and never flying in a direct line above thirty or forty yards, and never, when at the fartheſt, above a hundred yards diſtant from the rocks; cloſer to which they now, as the Sun lowered, began to gather very faſt. Their numbers now leſſened conſiderably; and in a very ſhort time they all returned into the fiſſures of the rocks, from whence they had been induced to venture out by the warmth of the evening. I was particularly careful to obſerve if there was a ſwallow amongſt them; but there was not one. Of this I am certain; for they were ſeveral times within the diſtance of twenty yards from the places where I ſtood. I was the more attentive to this, as I had been repeatedly aſſured, by many maſters of veſſels in the fiſh-trade, that they conſtantly ſaw every autumn, as they ſailed up the Mediterranean, vaſt flights of ſwallows, bending their courſe towards the South. From which there is the ſtrongeſt reaſon to believe, that theſe birds, during the winter months, do ſeek a warmer climate; though Mr. BUFFON has, for want of poſitive evidence, left that point undetermined. The above account, of which I am at all times ready to atteſt the truth, ſettling the queſtion, relative to martins, beyond any doubt, is the beſt apology I can make for the liberty I have taken in troubling you with it. If I have not been ſufficiently circumſtantial in this relation, I ſhall be at any time willing to anſwer any queries you may think proper to favour me with. I am, &c.

LETTER II.

TO THE HON. DAINES BARRINGTON.

SIR, March 31, 1775.

I SHOULD not have deferred a ſingle poſt acknowledging the receipt of your favour of the 19th *ult.* if I could ſo ſoon have procured a ſecond peruſal of your very intereſting paper, publiſhed in the LXIId volume of the Philoſophical Tranſactions, in which you have thrown ſo much light on the ſubject, from the inveſtigation of which I derive the honour of your correſpondence. The ſeaſon, as you obſerve, is arrived when ſwallows and martins do uſually begin to make their appearance; but there has not yet been one ſeen in this part of the country, probably from the uncommon coldneſs of the ſeaſon; but you may depend on my taking every method in my power to fulfil your requeſt, ſo far as to get ſome of the birds ſhot, as ſoon as they begin to iſſue forth from the rocks, where they have undoubtedly remained in a torpid ſtate ſince the month of November laſt. I am informed by a perſon who under-ſtands blowing rocks, that it is almoſt impoſſible to come at the bottom of the fiſſures of theſe in queſtion. It could not be done but by gun-powder, and then at a great expence, and not without danger of deſtroying much of the field above; beſides, the force of the powder pervading every hollow, would inevitably annihilate the birds, and ſo fruſtrate the end of our labour. I conceived the idea of deſtroying the rock in its full force, at the time when the martins entered it, as I concluded, for their winter's dormitory; and believe, that had the weather continued favourable, I ſhould have actually attempted it: but on reflection it did not appear, that the diſcovery would have been adequate to the difficulty and expence that would have attended it. For there is certainly nothing more extraordinary in finding martins, in a ſtate of torpidity, than dormice or bats, which are animals equal in bulk to the ſwallow or martin. Dormice are frequently found dead to all appearance in the winter in old hedges; and we can procure bats at all times, in any number, from a ſubterraneous place, called Kent's Hole, near Torbay. Now if the examination of the inteſtinal tube of one of this tribe of mungrel animals in a torpid ſtate, ſhould be thought worth attending to, it can be done at any time. Bats, indeed, are ſometimes ſeen in winter, in very mild weather; though none have yet made their appearance with us. And I am ready to atteſt, if occaſion ſhould require, that I have ſeen martins in Totneſs in the months of December and January; though I do not remember ever to have ſeen a ſwallow in the winter. Upon the whole, I cannot help thinking my own evidence, with reſpect to the martin, to be abſolutely concluſive, as is likewiſe the teſtimony of Mr. STEVENS and Dr. PYE; though it is to be regretted, that theſe gentlemen ſhould have left any doubt, whether the birds found in the mud were ſwallows or martins? And Mr. KLEIM, in his paper *De Hibernaculis Hirundinum*, aſſerts, that his father found three black martins or ſwifts in an old oak during the winter, which on being laid before a fire, ſoon recovered ſtrength enough to fly about the room, though they died ſoon after. The objection which has been brought againſt the opinion, that theſe birds do remain torpid during winter, is, that all birds do moult once in a year, and ſwallows do not moult with us. Now this argument is of little weight with me; as I am of opinion, that no bird that is to remain in a torpid ſtate during winter, can undergo the proceſs of moulting; for it is probable, if I may hazard ſuch a conjecture, that the great loſs of blood, which other birds ſuffer during the change of their feathers, is ſaved by nature, in birds which undergo a ſtate of torpidity, for their more effectual preſervation in ſuch a ſtate. And I have known many inſtances of birds kept in cages that have not moulted for a ſeaſon; particularly a ſky-lark, which retained his ſong in full vigour during the autumn and all the winter. Attempts have been made to bring on a torpid ſtate on the birds in queſtion, by confining them in a cold cellar: but without ſucceſs. The force of this objection ſeems to be loſt when we conſider, that in this ſituation the birds muſt be in continual fear, and conſequently not diſpoſed to make that change, to which inſtinct has directed them, for their preſervation and ſecurity; ſince all their attempts are to get out of confinement, as long as they have any ſpirit or ſtrength left; and when theſe are exhauſted, they die in courſe: and, I think, this ſtate muſt be induced by a diſpoſition in the animal itſelf, and cannot be brought on by compulſion. I hope I ſhall be ſucceſsful in my endeavours to procure ſome of the martins at their firſt appearance; as the being any way inſtrumental to oblige you will be a very great ſatisfaction to, &c.

LETTER III.

TO THE HON. DAINES BARRINGTON.

SIR, Totneſs, May 9, 1775.

I CAN make no other apology for having ſo long delayed writing to you, but a deſire to get as much information, as would make my letter worthy your peruſal; and if facts, as well eſtabliſhed as the nature of things can well admit, are allowed to be of any conſequence, I hope I have not entirely failed of ſucceſs. Mr. TRIST, the preſent recorder, and late member of parliament for this town, aſſures me, that he once ſaw many martins in the winter, about Chriſtmas, flying to and fro under a large rock, not more than a mile from Totneſs, and alſo near the river. Mr. DEVER, a reputable farmer, is ready at any time to make oath, that he once found a ſwift in the church of Aſhprington, in the middle of winter; that he took it in his hand, and though it ſhewed no ſigns of life, he is certain, it could not have been dead but a few hours. He ſuppoſes it dropped from the roof, at a time when ſome maſons were at work, repairing a breach. THOMAS DIDHAM alſo affirms, that he once ſaw, on the 26th of December, two ſwallows or martins, flying in a gentleman's court of Syfferton; that it was a pleaſant day; and that he then ſuppoſed that they had iſſued from the old thatch-covering of the out-houſes. But here follows a direct evidence, as to the torpidity of one kind of bird. Mr. WIAT made oath, laſt Sunday, in the pariſh church of Haberton, before me and a creditable witneſs, that in the winter, and near Chriſtmaſs, he once found, in a hollow aſh-tree then taking down, a bird covered with a kind of down; that on handling it, it ſhewed ſigns of life; that the two labourers who aſſiſted in felling the tree, alſo handled it; that when they firſt perceived the bird, it appeared to be dead, yet the heat of their hands made it move briſkly; and that this bird he believes to have been a cuckow. As the ſtory of the cuckow plucking off his feathers, and remaining torpid during the winter in hollow trees, is generally believed in this country, the eſtabliſhment of the fact appeared to me of conſiderable importance; and if I have ſucceeded in this point, I hope you will have no objection to the manner of aſcertaining it. I would wiſh to avoid a weak credulity on one hand, and obſtinate ſcepticiſm on the other. Again, Mr. ACHARD, of Privy Garden, may be now living, to teſtify the truth of the account of the torpid martins, which he ſaw taken out of the banks of the Rhine, and which, in his letter to P. COLLINSON, eſquire, read before the Royal Society, he ſo particularly deſcribes. Now to prove the torpidity of birds, we have the preſumptive evidence of Mr. TRIST, DEVER, DIDHAM, and myſelf; and the poſitive evidence of Mr. ACHARD, Dr. PYE, Mr. STEVENS, and Mr. WIAT; all men of character, and incapable of aſſerting an untruth. The opinion of ARISTOTLE, that ſome of the ſame ſpecies of birds do emigrate; and that others do pluck off their feathers, and remain torpid during the winter, cannot obtain credit: for we cannot ſuppoſe that theſe animals are governed by different inſtincts, in what immediately concerns their exiſtence; but by the ſame univerſal law of nature, independent of their wills or inclination. But then the law of nature muſt be permitted to have its free courſe. Reſtraint deſtroys the rule of actions; and therefore, though M. BUFFON and others ſucceeded not in their attempts to bring on torpidity in ſwallows confined in cold cellars, yet I have pleaſed myſelf with the idea of confining young ſwallows, martins, &c. with the old ones; which may be eaſily taken at their neſts when feeding, in a large walled garden, covered with a net or lattice, in which there ſhould be a pond. Probably the young birds, if the garden were large enough to find them food, would not be uneaſy under their confinement. If this experiment ſucceeded, it would be curious to obſerve them in the different degrees of torpidity during the winter. The inteſtines might at different times be inſpected, and their analogy with thoſe of the torpid bat carefully obſerved. I have had an opportunity of examining the *viſcera* of ſeveral torpid bats. The inteſtinal tube was perfectly empty, except about half an inch from the *anus*, where there was a little hard *fæces*. The gall-bladder was filled with a pellucid, yellowiſh fluid. The ball of FAHRENHEIT's thermometer being laid in the body of one of them, the heat of the blood at the heart raiſed the quickſilver two degrees. In three others, opened at the ſame time, no heat could be perceived, either by the thermometer or by the touch. Theſe experiments being made in the beginning of April, it is reaſonable to ſuppoſe, that the bat which affected the thermometer, had begun to feel the approaching ſeaſon. I think there is reaſon to believe, from the ſmall quantity of *fæces* in the inteſtines, and from its being ſo near the *anus*, that thoſe animals, when they find themſelves growing torpid, take ſufficient food to ſerve them during the winter. All the animal functions in this ſtate are carried on exceedingly ſlow; but that they do go on, in ſome degree, is evident from their emptineſs, emaciation, and the *fæces*, which are found in plenty underneath the place where they hang in cluſters.

The birds, of the ſwallow tribe(a), which I have procured, exhibit perfect plumage and extreme leanneſs; the inteſtines empty, except the gizzard, which contained a ſubſtance moſt like ſmall twigs or ſtraws. Swifts have not yet made their appearance with us; the firſt that can be procured ſhall be examined. The reſult ſhall be the ſubject of a future letter, when I hope once more to have the honour to ſubſcribe myſelf, &c.

(a) N. B. I had deſired him to ſhoot ſome of thoſe which firſt appeared, and examine their inteſtines; as I alſo did with regard to the torpid bats.

D. BARRINGTON.

XXXIV. *Deſcription and Uſe of a portable Wind Gage.* *By Dr.* James Lind, *Phyſician, at* Edinburgh.

Redde, May 11, 1775.

THIS ſimple inſtrument conſiſts of two glaſs tubes AB, CD, of five or ſix inches in length (TAB. X. fig. 1.). Their bores, which are ſo much the better always for being equal, are each about $\frac{4}{10}$ths of an inch in diameter. They are connected together, like a ſiphon, by a ſmall bent glaſs tube *ab*, the bore of which is $\frac{1}{10}$th of an inch in diameter. On the upper end of the leg AB there is a tube of latten braſs, which is kneed or bent perpendicularly outwards, and has its mouth open towards F. On the other leg CD is a cover, with a round hole G in the upper part of it, $\frac{2}{10}$ths of an inch in diameter. This cover and the kneed tube are connected together by a ſlip of braſs *cd*, which not only gives ſtrength to the whole inſtrument, but alſo ſerves to hold the ſcale HI. The kneed tube and cover are fixed on with hard cement or ſealing wax. To the ſame tube is ſoldered a piece of braſs *e*, with a round hole in it, to receive the ſteel ſpindle KL, and at *f* there is juſt ſuch another piece of braſs ſoldered to the braſs hoop *gh*, which ſurrounds both legs of the inſtrument. There is a ſmall ſhoulder on the ſpindle at *f*, upon which the inſtrument reſts, and a ſmall nut at *i*, to prevent it from being blown off the ſpindle by the wind. The whole inſtrument is eaſily turned round upon the ſpindle by the wind, ſo as always to preſent the mouth of the kneed tube towards it. The end of the ſpindle has a ſcrew on it; by which it may be ſcrewed into the top of a poſt, or a ſtand made on purpoſe. It alſo has a hole at L, to admit a ſmall lever for ſcrewing it into wood with more readineſs and facility. A thin plate of braſs *k* is ſoldered to the kneed tube, about half an inch above the round hole G, ſo as to prevent rain from falling into it. There is likewiſe a crooked tube AB (fig. 2.), to be put on occaſionally upon the mouth of the kneed tube F, in order to prevent rain from being blown into the mouth of the wind-gage, when it is left out all night, or expoſed in the time of rain. The force or *momentum* of the wind may be aſcertained by the aſſiſtance of this inſtrument, by filling the tubes half-full of water, and puſhing the ſcale a little up or down, till the 0 of the ſcale, when the inſtrument is held up perpendicularly, be on a line with the ſurface of the water, in both legs of the wind-gage. The inſtrument being thus adjuſted, hold it up perpendicularly, and turning the mouth of the kneed tube towards the wind, obſerve how much the water is depreſſed by it in the one leg, and how much it is raiſed in the other. The ſum of the two is the height of a column of water which the wind is capable of ſuſtaining at that time; and every body that is oppoſed to that wind, will be preſſed upon by a force equal to the weight of a column of water, having its baſe equal to the ſurface that is oppoſed, and its height equal to the altitude of the column of water ſuſtained by the wind in the wind-gage. Hence the force of the wind upon any body where the ſurface oppoſed to it is known, may be eaſily found; and a ready compariſon may be made betwixt the ſtrength of one gale of wind and that of another, by knowing the heights of the columns of water, which the different winds were capable of ſuſtaining. The heights of the columns in each leg will be equal, provided the legs are of equal bores; but unequal, if their bores are unequal. For ſuppoſe the legs equal, and the column of water the wind ſuſtains to be three inches, the water in the leg, which the wind blows into, will be depreſſed one inch and a half below o, and raiſed juſt as much above it in the other leg. But if the bore of the leg which the wind blows into, be double that of the other, the water in that leg will be depreſſed only one inch, whilſt it is raiſed twice as much, or two inches, in the other; and *vice verſâ*, if the ſame wind blow into the ſmaller leg, it will depreſs the water in it two inches, whilſt it raiſes it only one inch in the other. The force of the wind may be likewiſe meaſured with this inſtrument, by filling it until the water runs out at the hole *c*. For if we then hold it up to the wind as before, a quantity of water will be blown out; and, if both legs of the inſtrument are of the ſame bore, the height of the column ſuſtained, will be equal to double the column of water in either leg, or the ſum of what is wanting in both legs.

But if the legs are of unequal bores, neither of theſe will give the true height of the column of water which the wind ſuſtained. But the true height may be obtained by the following *formulæ*.

Suppoſe that after a gale of wind, which had blown the water in one of the tubes from A to B (fig. 3.), forcing it at the ſame time through the other tube out at E, the ſurface of the water ſhould be found ſtanding at ſome level DG, and it were required to know what was the height of the column EF or AB, which the wind ſuſtained. In order to obtain which, it is only neceſſary to find the height of the columns DB or GF, which are conſtantly equal to each other: for either of theſe added to one of the equal columns AD, EG, will give the true height of the column of water which the wind ſuſtained.

CASE I.

Let the diameters AC, EH, of the tubes be reſpectively repreſented by c, d; and let a=AD or EG, and x=DB or GF. Then it is evident, that the column DB is to the column EG as c^2x to d^2a. But theſe columns are equal. Therefore, $c^2x=d^2a$; and conſequently, $x=\frac{d^2a}{c^2}$.

EXAMPLE.

If the diameters AC, EH, be reſpectively 10 and 1, and AD or EG=3,96 inches, x will be=,0396 of an inch. For $d^2a=1\times 3{,}96=3{,}96$, which divided by $c^2=100$, gives $x=,0396$.

CASE II.

But if at any inſtant of time, whilſt the wind was blowing, it was obſerved, that when the water ſtood at E, the top of the tube out of which it is forced, it was depreſſed in the other tube to ſome given level BF, the altitude at which it would have ſtood in each, had it immediately ſubſided, may be found in the following manner:

Let b=AB or EF. Then it is evident, that the column DB is equal to the difference of the columns EF, GF. But the difference of theſe columns is as d^2b-d^2x. Therefore $c^2x=d^2b-d^2x$; and conſequently, $x=\frac{d^2b}{c^2+d^2}$.

For the caſes when the wind blows in at the narrow leg of the inſtrument.

Let AB=EF=b, EG or AD=a, GF=DB=x, and the diameters EH, CA, reſpectively =d, c, as before. Then it is evident, that the column AD is to the column GF as ac^2 to d^2x. But theſe columns are equal. Therefore, $d^2x=ac^2$; and conſequently, $x=\frac{ac^2}{d^2}$. This anſwers to CASE I.

It is alſo evident, that the column AD is equal to the difference of the columns AB, DB. But the difference of theſe columns is as bc^2-c^2x. Therefore, $d^2x=bc^2-c^2x$. Whence we get $x=\frac{bc^2}{d^2+c^2}$. This correſponds to CASE II.

As there is always a calculation to be made for every experiment when the legs of the inſtrument are of unequal bores, I would recommend it to the makers of theſe inſtruments, to make uſe of tubes that are equal, or at leaſt nearly ſo, that the error may become next to nothing, it being a thing very eaſy to be done. In this manner we can readily determine the greateſt force, which the wind has blown with, during the time the inſtrument has been expoſed to its action. But as it may be ſafely left alone, by ſcrewing its ſpindle into the proper ſtand, or into the top of a poſt, and as the wind never fails to turn the mouth of it towards itſelf, it is not neceſſary for the obſerver to continue always by it; for it may be allowed to ſtand all night, expoſed to the wind, without any inconvenience, though it ſhould even happen to rain very heavily. However, recourſe can only be had to this method of uſing the inſtrument on ſhore: for at ſea it muſt always be held up in a perpendicular poſition in the hand, whether it be uſed when only half full of water, or when quite full; which laſt will be frequently found to be the only practicable method of aſcertaining the force of the wind during the night, when it blows ſo hard that it is impoſſible to keep any lights on deck. A perſon filling the wind-gage, in a calm place, with water, in order to determine the force of the wind, in the way which I have been juſt now deſcribing, will be apt to imagine, that it cannot give the meaſurement correct; for he will find ſuch a repulſion to ariſe from the edges of the hole G, as to ſuſtain a column of water in the kneed or bent tube, perhaps half an inch above the level;

level: but by either blowing acroſs the round hole, or moving his finger over it, he will ſoon bring the water in the kneed tube to ſtand at the ſame level with it, by taking off gradually the convex ſuface of the water, which projects out at the hole in the form of a drop or *ſpherule*. And this effect the wind very ſoon produces itſelf. There ought always to be a cover on the top of the tube out of which the water is expelled by the wind; but it ſhould be made very thin. For if there be no ſuch cover, and the mouth of the kneed tube be ſtopped, after the inſtrument is quite full of water, in order to prevent the wind from having any influence in raiſing it, you will find, upon expoſing it to a ſtrong gale, that in a very ſhort time it will blow out perhaps half an inch of water. Whence it appears, that a very conſiderable error would ariſe from uſing the wind-gage in this ſtate. But in all the experiments which I have made with this inſtrument, whilſt it had the cover and the round hole of $\frac{2}{10}$ths of an inch in diameter in the middle of it, I have not been able to diſcover any error. The uſe of the ſmall tube of communication *ab* (fig. 1.) is to check the undulation of the water, ſo that the height of it may be read off from the ſcale with eaſe and certainty. But it is particularly deſigned, to prevent the water from being thrown up to a much greater or leſs altitude, than the true height of the column, which the wind is able at that time to ſuſtain, from its receiving a ſudden impulſe, whilſt it is vibrating either in its aſcent or deſcent. For water in the legs of a ſiphon is capable of being put into a vibrating motion like a pendulum (a); and therefore, if acted upon when in the aſcent, the height which it aſcends to will come out greater than the truth; and leſs, if acted on in the deſcent.

The height of the column of water ſuſtained in the wind-gage being given, the force of the wind upon a foot ſquare is eaſily had by the following table, and conſequently on any known ſurface.

TABLE I.

Height of the water in the wind-gage.	Force of the wind on the foot ſquare in avoirdupois pounds.	Common deſignation of ſuch a wind.
12 inches	62,5	
11	57,293	
10	52,083	moſt violent hurricane.
9	46,875	
8	41,667	very great ditto.
7	36,548	great hurricane.
6	31,75	hurricane.
5	26,041	very great ſtorm.
4	20,833	great ditto.
3	15,625	ſtorm.
2	10,416	very high wind.
1	5,208	high wind.
$0\frac{1}{2}$	2,604	briſk gale.
$0\frac{1}{10}$	,521	freſh breeze.
$0\frac{1}{20}$	,260	pleaſant wind.
$0\frac{1}{40}$	,130	a gentle wind.

(a) NEWTONI Princip. Mathemat. lib. II. prop. XLIV. theor. XXXV.

EXAMPLE.

EXAMPLE.

If it were required to know the force of the wind, when the column of water ſuſtained was equal to $4\frac{6}{10}$ inches. Then, by TAB. I.

Pounds.
4 inches = 20,833
0,5 or ½ inch = 2,604
0,1 = 0,521
Sum 4,6 = 23,958 = force on every ſquare foot.

Any change that can happen in the ſpecific gravity of the water from heat or cold, will make no ſenſible alteration on experiments made with this inſtrument.

A cubic foot of water is generally ſuppoſed to weigh 1000 Avoirdupois ounces; and from ſome experiments made by Mr. MUSSCHENBROEK it would appear, that betwixt freezing and boiling, or in 180° on FAHRENHEIT's ſcale, it increaſes only $\frac{1}{85}$ = ,0117 of its whole bulk, or volume(b). I cannot, however, find any author that mentions at what preciſe degree of heat a cubic foot of water was weighed. Mr. FAHRENHEIT indeed made ſeveral of his curious experiments on the ſpecific gravities of bodies when the water raiſed his thermometer to 48°(c). Now if we ſuppoſe the greateſt heat of the water which we make uſe of in the wind-gage to be 90°, which exceeds 48° by 42, the greateſt change produced will be only ,0027 or $\frac{27}{10000}$ parts of the whole. So that if the altitude of the column of water ſuſtained by the wind were even to be five inches, the part of this effect, ariſing from the diminution of the ſpecific gravity of the water, occaſioned by the greateſt heat, will only amount to 0,0135, or $\frac{135}{10000}$ parts of an inch, a change which cannot be meaſured by the inſtrument. It may be ſometimes neceſſary to employ other fluids beſides water, particularly if the degree of cold be below freezing: for then we muſt uſe a fluid that will not freeze in the degree of cold in which we expoſe the inſtrument, otherwiſe the wind can have no influence on it, and the liquor freezing in the tube will break it. I ſhall, therefore, mention a few liquors in the following table that will anſwer the purpoſe, as alſo ſubjoin a general method of reducing them all to one common meaſure. But of all the fluids I am acquainted with, when the effects of froſt are to be feared, I know none better adapted to our purpoſe than a ſaturated ſolution of ſea-ſalt; ſince it does not freeze till the thermometer falls to 0 degrees, and is a fluid conſtantly of the ſame ſpecific gravity. Spirit of wine, independent of its being more variable in reſpect of ſpecific gravity by the influence of heat and cold, is alſo more or leſs ſo, as it is more or leſs rectified. And although the true ſpecific gravity were known at the beginning of the operation, it would even change during the time of uſing it, by imbibing moiſture from the air.

(b) MUSSCHEN. Introd. ad Philoſ. Natur. tom. II. p. 625.

(c) Philoſophical Tranſactions, N° 383.

Let *w* repreſent the weight of a column of water, having its altitude meaſured by one of the diviſions on the ſcale, and its baſe to any given ſurface whatever; and let *n* denote in general the number of theſe diviſions which meaſures the whole length of the column of the water which the wind ſuſtains. Then *nw* will repreſent always its weight, and will ſerve as a common multiplier for the ſpecific gravities of all other liquors.

TABLE II.

Names of liquors.	Specific gravities.	Common multiplier.	Weight meaſuring the forces of the wind.
Water,	1,000	nw	nw
Sat ſol. of ſalt,	1,244		$1,244 \times nw$
Urine,	1,030		$1,030 \times nw$
Ditto,	1,016		$1,016 \times nw$
Alkohol,	0,825		$0,825 \times nw$
Proof ſpirits,	0,927		$0,927 \times nw$
	&c. &c.		&c. &c.

EXAMPLE.

Let *w* repreſent the weight of a column of water $\frac{1}{20}$th of an inch high, ſtanding on a ſquare foot; and let $n=80=4$ inches. Then (by TAB. I.) nw is equal to 20,833 Avoirdupois pounds. Therefore $1,244 \times 20,833$ =weight of a ſaturated ſolution of ſea ſalt of the ſame altitude. and $\frac{4}{1,244}$ = the altitude of a column of a ſaturated ſolution of the ſame, weighing 20,833 pounds Avoirdupois. w may repreſent a ſquare yard, the ſurface of a ſail, &c.

If the velocity and denſity of the wind in any particular caſe were accurately determined, this inſtrument, which gives its force or *momentum*, would enable us to aſcertain the velocity in every other caſe, the denſity being known. For it appears from experiments, made by Mr. JAMES FERGUSON, F. R. S. on the whirling-table, that its force is as the ſquare of its velocity. But as the denſity, which is one of the *data* requiſite for determining the velocity by this inſtrument, was not taken into conſideration in theſe experiments, all that we can do at preſent is to ſuggeſt the idea.

It may not, perhaps, be improper to take notice, that evaporation will have ſome effect in diminiſhing the altitude of the column of water; though its influence, for the moſt part, will be very inconſiderable. The more frequently, therefore, the inſtrument is examined, it will be ſo much the better. If it be expoſed to the action of the wind, whilſt it happens to ſnow, it will be neceſſary to look at it frequently, leaſt the ſnow ſhould choak up the mouth of the wind-gage.

Extract of a letter from Dr. LIND to Col. ROY. Dated Edinburgh, May 26, 1775.

I ght to be ſomewhat longer than that I lately ſent Sir JOHN PRINGLE. For we had a gale here on the 9th current, which ſupported a column of water of

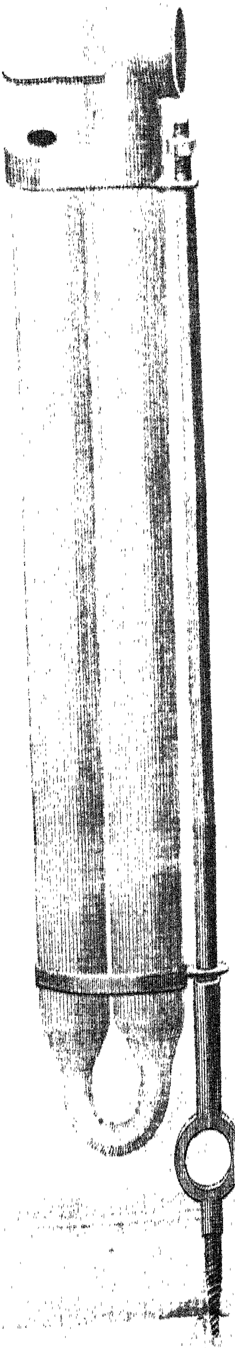


Fig. 2.

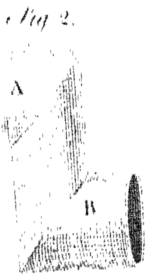


Fig. 3.

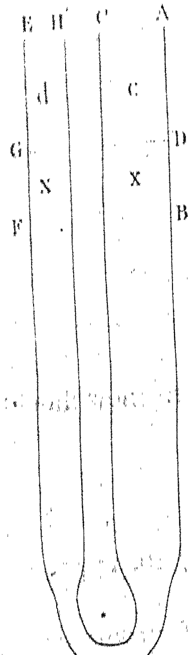
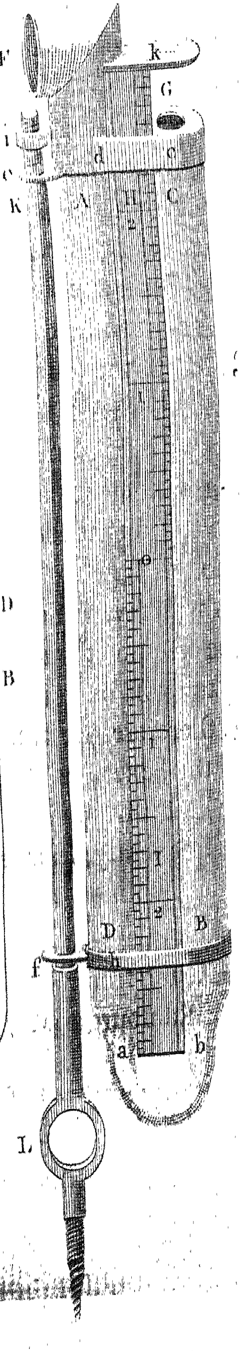


Fig. 1.



of $6\frac{7}{10}$ inches, whereas that I ſent was not ſo long. The force of this gale on a ſquare foot was equal to 34,921 pounds Avoirdupois, and it has done great damage to our gardens. Weſt India hurricanes would require gages of a ſtill greater length to meaſure them.

XXXV. *Aſtronomical Obſervations made at* Leiceſter. *By the Reverend Mr.* Ludlam, *Vicar of* Norton, *near* Leiceſter. *Communicated by the* Aſtronomer Royal.

Redde, May 11, 1775.

OBSERVATIONS FOR DETERMINING THE LATITUDE OF THE PLACE.

Zenith diſtances taken with an eighteen inch quadrant, made by BIRD.

1774.	β Draconis on quadrantal arch				γ Draconis on quadrantal arch.					
	Degrees M.	S.	Parts. S.	V.	Degrees. D.	M.	S.	Parts of 96. P.	S.	V.
July 2	9	32	1	$5\frac{1}{2}$	1	6	40	1	1	$7\frac{3}{5}$
10	9	20	1	$5\frac{1}{4}$	1	6	34	1	1	$7\frac{1}{8}$
14	9	20	1	$5\frac{1}{3}$	1	6	34	1	1	$7\frac{1}{2}$
15	9	20	1	$5\frac{1}{6}$	1	6	28	1	1	$7\frac{1}{4}$
20	9	20	1	$5\frac{1}{8}$	1	6	27	1	1	$7\frac{1}{5}$
Mean	9	22,4	1	$5\frac{11}{40}$	1	6	32,6	1	1	$7\frac{41}{100}$
	On arch of exceſs.				On arch of exceſs.					
	M.	S.	S.	V.	D.	M.	S.	P.	S.	V.
June 30	8	58	1	$4\frac{1}{4}$	1	6	12	1	1	$6\frac{1}{4}$
July 4	8	50	1	$4\frac{1}{8}$	1	6	6	1	1	$6\frac{1}{3}$
9	8	58	1	$4\frac{1}{6}$	1	6	4	1	1	$6\frac{1}{2}$
16	8	58	1	$4\frac{1}{6}$	1	6	6	1	1	$6\frac{1}{2}$
18	8	58	1	$4\frac{1}{8}$	1	6	6	1	1	$6\frac{1}{3}$
Mean	8	56,4	1	$4\frac{1}{6}$	1	6	6,8	1	1	$6\frac{22}{60}$

N.B. The ſeconds were ſhewn by the micrometer ſcrew, the fractional parts eſtimated by the eye.

Reduce the parts of 96 to degrees, and take the mean between the zenith diſtances ſhewn on each ſcale, and the zenith diſtance of β Draconis on the quadrantal arch will be 9′ 21″,7, and on the arch of exceſs 8′ 54″, whence the true zenith diſtance is 9′ 7″,8, and the error of the line of collimation 13″,8, to be ſubtracted from the numbers ſhown on the limb of the quadrant. In like manner we ſhall find the true zenith diſtance of γ Draconis 1° 6′ 19″,8, and the error of the line of collimation 13″,5. If we ſuppoſe the apparent declination of β Draconis on July 12th to be 52° 28′ 52″,3, that of γ Draconis 51° 31′ 41″,7, we have the latitude from the former 52° 38′, and from the latter 52° 38′ 1″.

N. B. Some obſervations on theſe two ſtars in July 1772, give the ſame latitude within leſs than 2″, but make the error of the line of collimation 23″ to be ſubtracted. I ſuſpect the line of collimation is liable to ſmall variations in portable quadrants, if not in all.

Zenith diſtances of α Herculis with the ſtate of the barometer and thermometer.

1774.	Degrees.			Parts of 96.			Barom.	Therm.
	D.	M.	S.	P.	S.	V.	Inches.	Degrees.
June 30	37	57	36	40	3	14$\frac{1}{2}$	29,7	58
July 2	37	57	46	40	3	14$\frac{3}{4}$	29,8	65
4	37	57	40	40	3	14$\frac{1}{2}$	29,7	56
9	37	57	41	40	3	14$\frac{2}{3}$	29,7	55
10	37	57	36	40	3	14$\frac{1}{3}$	29,7	55
16	37	57	36	40	3	14$\frac{1}{3}$	30,0	65
18	37	57	32	40	3	14$\frac{1}{4}$	30,0	56
20	37	57	32	40	3	14$\frac{1}{3}$	29,8	58
Mean	37	57	37,4	40	3	14$\frac{11}{24}$	29,8	58,5

The mean of the zenith diſtances ſhewn on the two ſcales of diviſions is 37° 57′ 32″. Add for refraction 43″,4: ſubtract for line of collimation 13″,6: and we have the true zenith diſtance 37° 58′ 1″,8. Suppoſe the apparent declination of α Herculis on July 12th to be 14° 39′ 58″,5, we have the latitude 52° 38′.

Zenith

Zenith diſtances of the Pole-Star.

1774.	Degrees.			Parts of 96.			Barom.	Therm.
	D.	M.	S.	P.	S.	V.	Inches.	Degrees.
Nov. 10	35	27	56	37	6	$9\frac{7}{8}$	30,05	31
11	35	27	55	37	6	$9\frac{1}{2}$	29,87	32
13	35	27	56	37	6	$9\frac{7}{8}$	30,20	34
Mean	35	27	55,6	37	6	$9\frac{3}{4}$	30,04	32,3
Dec. 6	35	28	8	37	6	$10\frac{1}{8}$	30,27	30
15	35	28	4	37	6	$10\frac{1}{4}$	30,03	44
Mean	35	28	6	37	6	$10\frac{3}{16}$	30,15	37
Dec. 12	39	15	0	41	6	$14\frac{3}{4}$	29,83	44
13	39	15	4	41	6	15	28,86	44
15	39	15	0	41	6	$14\frac{3}{4}$	30,08	38
Mean	39	15	1,3	41	6	$14\frac{5}{6}$	29,59	42

Take the mean between the two ſcales of diviſions, and we have the mean zenith diſtances, as follows:

Days of the month.	Obſ. zenith diſt.			Cleared of refr.		
	D.	M.	S.	D.	M.	S.
Nov. 10 11 13	35	27	49,5	35	28	33,5
Dec. 6 15	35	28	0,3	35	28	44,
Dec. 12 13 15	39	14	59,3	39	15	46,7

The mean zenith diſtance of Nov. 10. 11. 13. cleared of refraction, is 35° 28′ 33″,5. To this add 7″,5 for the increaſe of apparent declination between Nov. 12. and Dec. 12. and we have the zenith diſtance on Dec. 12. (as derived from the obſervations in November) 35° 28′ 41″.

The ſame from the actual obſervations on Dec. 6. and 15. is 35° 28′ 44″. The mean of theſe two, corrected for the line of collimation, gives the true zenith diſtance above the pole, 35° 28′ 28″,9. The obſervations of Dec. 12. 13. 15. cleared of refraction and corrected for the line of collimation, give the true zenith diſtance below the pole, 39° 15′ 33″,1, whence the latitude 52° 37′ 59″, and the apparent declination of the pole ſtar, Dec. 12th, 88° 6′ 27″,9. From all theſe obſervations we may conclude the latitude of (St. Martin's church in) Leiceſter is 52° 38′, within very few ſeconds (a). From ſome obſervations made with an HADLEY'S quadrant of ſix inches radius, and given in the Tranſactions for 1769, I made the latitude only 52° 37′ 3″; but thoſe obſervations cannot be ſet in competition with theſe, either for weight or number.

Occultations of γ and α Tauri, obſerved at Leiceſter, Nov. 18, 1774.

		Time by the clock.		
		H.	M.	S.
Emerſion γ Tauri,		VI	27	10
Immerſion α Tauri.	Touched the limb,	XIV	59	26
	Vaniſhed,	XIV	59	30
Emerſion α Tauri inſtantaneous,		XVI	12	38

(a) The obſervations were made in Wigſton's hoſpital adjoining to the church.

The following obſervations ſerve to examine the clock.

Tranſits of the Sun.

Day of the Month.	1ſt wire.	Time of the Clock. Meridian.	3d wire.
1774.	M. S.	H. M. S.	M. S.
Nov. 17	43 35	XXIII 44 $24\frac{1}{2}$	45 $11\frac{3}{4}$
	45 54	46 43	47 30
18	43 50	XXIII 44 39	45 26
	46 $8\frac{1}{2}$	46 57	47 44

Hence the *rate* of going was conformable to mean time.

Zenith diſtances taken with the eighteen inch quadrant, to aſcertain the abſolute error of the clock, Nov. 18. Barometer 29,6 inches. Thermometer 33°.

Time by clock.	Degrees.	Parts of 96.	Object.
H. M. S.	D. M.	P. S. V.	
VIII, 0 0	67 $11\frac{1}{2}$	71 5 6	α. Aquilæ.
2 46	67 $35\frac{7}{8}$	72 0 $13\frac{1}{2}$	
12 15	68 59	73 4 11	α Aquilæ.
14 37	69 20	73 7 $10\frac{1}{3}$	
48 4	56 $45\frac{3}{4}$	60 4 $5\frac{1}{3}$	α Tauri.
50 55	56 $20\frac{5}{6}$	60 0 $13\frac{1}{2}$	
53 23	55 $59\frac{1}{2}$	59 5 13	
XI 1 57	55 21	59 0 5	β Geminorum.
3 54	55 3	58 5 12	
6 35	54 39	58 2 5	
XVI 26 33	58 $45\frac{3}{5}$	62 5 7	α Tauri.
29 49	59 $14\frac{1}{3}$	63 1 8	
33 9	59 44	63 5 12	
36 15	60 $11\frac{1}{2}$	64 1 10	

From the mean of the two firſt zenith diſtances of α Aquilæ the clock will be found to be ſlower than mean time 14″. By the ſecond pair of α Aquilæ, 13″. By the next three zenith diſtances of α Tauri, 13″. By the next three of β Geminorum, 16″, By the laſt four of α Tauri, 17″; the error of the line of collimation being 13$''$,6 as before. The mean of all theſe gives the clock 14$''$,6 ſlower than mean time. Hence,

		Solar time. H. M. S.
Emerſion of γ Tauri,		VI 41 48,4
Immerſion α Tauri.	Touched limb,	XV 13 59,6
	Vaniſhed,	XV 14 3,6
Emerſion α Tauri,	inſtantaneous,	XVI 27 10,8

The emerſion of α Tauri was obſerved at Greenwich at xvi^{h} 34$'$ 36$''$,8 ſolar time.

N. B. The immerſion of ζ Tauri (behind the Moon) which was obſerved at Leiceſter, April 28, 1770, at IX^{h} 45$'$ 44$''$ ſolar time, was alſo obſerved at Greenwich at IX^{h} 51$'$ 28$''$,6 ſolar time. See Phil. Tranſ. for 1770.

XXXVI. *Remarks and Conſiderations relative to the Performance of Amputation above the Knee, by the ſingle circular Inciſion.* *By* Benjamin Gooch, *Surgeon at* Norwich.

Redde, May 19, 1775.

IN April, 1774, I was aſked, in conſultation with ſeveral other ſurgeons, in the caſe of a bad leg, in a lad of about fifteen years of age, belonging to the Belchamp-houſe of Induſtry, in the county of Suffolk. The propriety of the operation above the knee was indiſputable; and the ſingle, inſtead of the double, inciſion, which I propropoſed to the conſideration of my brethren, was unanimouſly aſſented to, and done in the following manner.

After drawing up the teguments as far as could be done, fixing them tight and very exactly with a narrow band (a), before ſtraitening the tourniquet ligature, the operation was performed by Mr. REVANS, of Haleſworth, who made the inciſion quick and with great exactneſs, quite round to the bone, ſteadily at once; rather lower poſteriorly and internally than anteriorly and externally, as the band directed, in conſideration of the different attachment of the muſcles. Theſe were then carefully diſſected from the *perioſteum*, and drawn up with the retractor, as I formerly deſcribed; ſo that after the uſe of the ſaw, the end of the bone was lodged full two inches in the muſcles(b). This manner of operation ſucceeded to our wiſh; leaving a flat, even ſtump, which was completely cicatrized in leſs than two months; and with but little exfoliation from the bones, as Mr. REVANS informed me: ſince that, he aſſured me, that it was the beſt ſtump he had ever ſeen, which he aſcribed to the manner of amputating, and the treatment afterwards.

(a) The band may be about an inch broad, made of firm linen cloth or thin leather, ſpread with common plaſter; and in order to increaſe its power, it may have a ſlit in the middle, and be uſed like the uniting bandage.

The following method in this caſe was uſed to keep down the teguments and muſcles, during the cure of the ſtump, which I have practiſed more than forty years, much to my own ſatisfaction, and the approbation of many of my brethren. As ſoon as the tenſion is over, and the wound well digeſted, I apply a circular plaſter, moderately adheſive, four or five inches broad, with great exactneſs, near to the edge of the wound; and then affix to the whole breadth of it, at due diſtances, ſix ſtraight ſtraps of the ſame kind of plaſter, of ſuitable dimenſions, with ſlits in half of them, for their oppoſites to be paſſed through over the neceſſary dreſſings; and an eaſy well-adapted compreſs of a ſufficient thickneſs, having a piece of ſtiff paper on the outſide of it; directing an aſſiſtant to thruſt gently and evenly down, with both hands, the teguments and muſcles, whilſt I draw the ſtraps properly ſtrait, and ſecure the ends of them when reflected, with a narrower circular plaſter. All this can be eaſily removed, and occaſionally renewed at the time of dreſſing, letting the other circular plaſter remain without renewal, as long as its adheſion effectually anſwers the purpoſe. And after proceeding in this manner, I generally find a cap ſufficient, without any bandage (c).

(b) See Chapter upon Amputations in my Caſes and Remarks in Surgery, edit. 2.; and what M. LOUIS ſays upon this ſubject, in the Mem. de l'Acad. Roy. de Chirurgie, tom. II. et IV. In the latter of which, this eminent ſurgeon mentions his having had compreſſion ſucceſsfully made upon the crural artery in the groin, in ſome amputations that he performed, where there was not room to apply the tourniquet to the thigh. The celebrated M. PETIT, many years ago, preſerved the life of a gentleman, by inventing a machine to ſerve ſuch a purpoſe, on account of a hæmorrhage, twenty-one days after having had his thigh amputated higher up than ordinary.

This is not the only inſtance in which I have had good reaſon to approve the ſingle inciſion; and ſhould it be found, by ſufficient experience, to anſwer the end in general, but as well as the double, in preventing a conical ſtump (d), as mentioned in my Caſes and Remarks in Surgery, it will indiſputably deſerve the preference; and though, from what I have obſerved, there ſeldom is much projection of the bone attending the double inciſion,

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(c) When I made ſome remarks upon amputation formerly, I ſhould have adviſed this method, from its great utility experienced in many inſtances.

(d) I have been informed, by good authority, that a reſection of the bone is no uncommon practice at Paris, to remedy this inconvenience.

when it has been properly made, and the cure of the wound judicioufly conducted; yet I have ſometimes obſerved, notwithſtanding the utmoſt judgement and attention, ſome appearance of want of muſcular ſubſtance to form a well-ſhaped ſtump. And in whatever method an amputation has been performed, a very ſtrait bandage ſhould by all means be avoided in the ſubſequent treatment of the wound, for obvious reaſons; namely, becauſe it obſtructs circulation, and is an impediment to nutrition. The condition of the patient, the ſtate of the limb, and the part of it where the operation is ſometimes found abſolutely neceſſary to be performed, with other concurring circumſtances may prevent the formation of a good ſtump under the moſt ſkilful management, whether the ſingle or double inciſion was practiſed, according as the ſurgeon ſhall find reaſon to determine his choice, by duly exerciſing his judgement in this nice point. Conſidering the different degrees of attachment and adheſion of the muſcles, by means of the cellular membrane, if kept firmly together with the ſtrait narrow band, as has been deſcribed, it is evident, they will be more evenly and regularly divided by the ſingle than by the double inciſion; for when the muſcles are laid bare, if a ſtimulus of any kind is applied to their fibres, it is evident, they will contract more or leſs, according to their different attachments, and different degrees of irritability: and for this reaſon an even ſection will not eaſily be effected, unleſs the whole inciſion be made at once, and as it were in an inſtant, ſo as not to allow ſufficient time for the irritation of the knife to take effect upon the bare muſcles(e).

(e) See two Diſſertations, lately publiſhed by my ingenious friend Dr. KIRKLAND, of Aſhby de la Zouch; the one upon the Brain and Nerves; and the other on the Sympathy of the Nerves, and Irritability. I am alſo inclined to think, and not without the authority of experience, that amputation below the knee, by the ſingle inciſion, may be found preferable; obſerving what has been mentioned, and cutting the *gaſtrocnemius* muſcle a little ſhorter than the other muſcles, as Mr. SHARP directs in his Chirurgical Operations.

XXXVII. *Con-*

XXXVII. *Concerning Aneuryſms in the Thigh.* *By* Benjamin Gooch, *Surgeon at* Norwich.

Redde, May 19, 1775.

IN the caſe deſcribed in the preceding paper, we obſerved a diviſion of the femoral artery into two trunks of equal ſize, running parallel, and ſo near together, as that we could conveniently include them in one ligature with the needle, avoiding the nerve, after raiſing them up with the diſſecting forceps, by a ſmall portion of the connecting cellular membrane. And here we found no occaſion to take up any other veſſel. This makes the third inſtance in amputations of the thigh, in which I have obſerved ſuch a *luſus naturæ* in the arterial ſyſtem; hence I am inclined to think it not improbable that this has often happened, though I do not find it has been noticed by any other ſurgeon. The remark may prove of ſome practical uſe, as I have hinted in my Treatiſe on Wounds, p. 78. in reſpect to aneuryſms in the thighs. It might indeed reaſonably be feared, where there is only one trunk of the femoral artery without a diviſion, which is commonly ſuppoſed to be the general caſe, that the lateral and communicating branches would not ſufficiently enlarge, to carry on the circulation of the blood, and preſerve the life and uſe of the limb after the operation for an aneuryſm.

This point ſeems worthy of particular conſideration; and indeed it proved the ſubject of ſome controverſy, in the caſe of an aneuryſm about four inches above the knee of a healthful young man (ſuppoſed to proceed from a fall a year before) in which my opinion was aſked. Some months after, one of the ſurgeons concerned in the conſultation told me, there appeared at this time no hopes of ſucceeding by the operation for the aneuryſm; and but little of ſaving the patient's life by amputation. It is not to be expected that an operation of this nature, ſo very precarious in this part, ſhould in every inſtance be attended with the ſucceſs, which Mr. LESLIE, an eminent ſurgeon at Corke, had the ſatisfaction to experience, in a caſe, which is related in the Medical and Philoſophical Commentaries of the Edinburgh Society, N° VI. p. 176. § 2. The ſurgeon, however, ought by no means to incur cenſure for the unfortunate event, after having taken all prudent and probable meaſures, to preſerve his patient's limb (a).

(a) The celebrated CYPRIANUS, who was profeſſor of anatomy and ſurgery in the univerſity of Francker, and the moſt famous lithotomiſt of his time, after ſucceſsfully performing the Cæſarian operation at Lewarden, in which he found the *fœtus* in the right Fallopian tube, wrote a letter, upon that occaſion, to Sir THOMAS MILLINGTON, phyſician to CHARLES II.; and afterwards he happily cut that eminent phyſician for the ſtone, at the age of ſixty-eight, in London. In which letter, he earneſtly admoniſhes ſurgeons not to be intimidated by threatening proſpects, from undertaking dangerous and difficult operations, leſt their reputations ſhould ſuffer for want of ſucceſs. This letter, written in French, conſiſts of ſeventy pages, containing much ſolid, practical knowledge and was annexed to BELLOSTE's 4th edition of his Chirurgien de l'Hôpital, printed at Amſterdam in the year 1707.

From theſe conſiderations, I communicated my thoughts upon this ſubject to ſome of my brethren, wiſhing to have experiments made upon brutes, that might aſcertain as far as poſſible, by analogy, a matter which appeared to us of great importance; and this motive induced one of them, with the aſſiſtance of a perſon of ſuperior knowledge in the anatomy and diſeaſes of horſes, to reſolve upon diſſecting out of the thighs of a horſe and a dog, the firſt opportunity that offered, the trunk of the artery to the length of two or three inches; to obſerve whether there was ſuch a diviſion of it, as I have remarked in the human ſpecies; and then to treat the wound as after the operation for an aneuryſm, attending particularly to all the conſequences. This kind of *luſus naturæ* has been often found in the *humerus*, by anatomiſts. After having engaged my friends at Norwich in this purſuit, ſomething happening to prevent their carrying the deſign into execution ſo ſoon as they intended, determined me, with the aſſiſtance of Mr. REVANS, whom I have mentioned before, to make the experiment upon a full-grown young ſpaniel, and to keep a journal of the occurrences in conſequence thereof.

On January the 19th, 1775, we performed the operation with very little trouble, having ſecurely bound the animal to prevent interruption. We deſignedly included in the ligatures, with the trunk of the artery, a little above the middle of the thigh, the vein and nerve accompanying it, in order to render the experiment more deciſive, if it ſucceeded, than it would have been, had the artery alone been taken in. We diſcovered no diviſion in the trunk of the artery like what I have obſerved; and having made the wound of a ſufficient extent, we ſucceeded at once in paſſing the ligatures with the needle, without the leaſt hæmorrhage. The wound was anointed with freſh hog's lard, to tempt the dog the more readily to lick it; and this application was often repeated, having at the ſame time the whole limb embrocated with *linimentum volatile*, which was well rubbed in. After the operation, the dog ſhewed no ſigns of great pain, had no ſpaſmodic motions in the limb, but made no uſe of it, and we could feel no pulſation below the ligatures. On the 20th, nothing appeared worthy notice, only that there was a warmth throughout the limb, nearly to the ſame degree as in the other. 21ſt, A little œdematous ſwelling appeared upon the leg; in other reſpects, as yeſterday. 22d, Same appearances as yeſterday; begun to move the limb. 23d, Moved the limb rather more; wound looked well. 24th, Moved the limb more than yeſterday; wound well digeſted; œdematous ſwelling leſs. 25th, The upper ligature came off; no *hæmorrhage* enſued. 26th, Wound in a healing ſtate; he began to ſtep upon the limb; œdematous ſwelling quite diſperſed. 27th, All appearances favourable. 28th, The other ligatures came off, without any oozing of blood. 29th, The wound contracted into a narrow compaſs, by virtue of his own balſamic tongue(b); no other application was made to the wound or limb than mentioned at firſt. 30th, No material alteration. 31ſt, He uſed the limb almoſt as well as the other. Feb. 9th, Dr. D'URBAN, of Haleſworth, who attended to the ſequel of this experiment, ſtrictly examined the limb with Mr. REVANS and me; and we could feel no pulſation of the trunk of the artery in the ſpace, nor in the lower part of the limb, after the wound had been perfectly healed ſome days. The dog was then remarkably briſk and lively, and as active as uſual, without any impediment in the motion of the limb; and no ſwelling remained in any part of it.

We kept the dog alive till the 25th of March, and examined the limb attentively from time to time, without finding any viſible defect in it, or want of ſenſation. Then, it not being likely that any thing more remarkable ſhould appear, while he lived, we had him killed for farther inquiry. We had an apparatus ready for injection; but upon conſideration, that carefully diſſecting out the parts where we had performed the operation, and then carefully examining them, might more effectually anſwer our intention, we omitted the uſe of it. We did not find the artery divided into two equal branches, as I have mentioned and deſcribed in the human ſubject; but a ramification evidently appeared to us much enlarged, by what we obſerved in the other limb, which departed from the trunk at an acute angle, juſt above where we paſſed the upper ligature in the operation; and the ſpace between the two ligatures was filled up with a fleſhy ſubſtance. Dr. D'URBAN was alſo preſent at this examination.

(b) See M. BELLOSTE's Diſſertation upon the healing Virtue of a Dog's Tougue, in vol. II. of his Hoſpital Surgeon. See alſo the note at p. 173. of my Caſes and Remarks in Surgery, edit. 2.

XXXVIII. *An Account of further Diſcoveries in Air. By the Rev.* Joſeph Prieſtley, *LL.D. F. R. S. in Letters to Sir* John Pringle, *Bart. P. R. S. and the Rev. Dr.* Price, *F. R. S.*

LETTER I.

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR, March 15, 1775.

Redde, May 25, 1775.

HAVING been pretty fortunate in the proſecution of my experiments on different kinds of air, ſince the publication of my treatiſe on that ſubject, I think it due to the attention with which you have from the firſt honoured them, to give you ſome account of what I have lately done. I know that every new diſcovery, in any branch of natural knowledge, gives you pleaſure; and it is peculiarly flattering to me, that you conſider ſome of thoſe, which I have been happy enough to make, in a light of ſome importance. As I have materials enough for another ſeparate publication, I ſhall not trouble the Society with a particular account of my obſervations; but if you think proper to communicate to them the following very general account, as a mark of my reſpect for the Society, as well as for yourſelf, you will add to the many obligations you have conferred upon me.

To the marine acid air, which I had diſcovered at the time of my former publication, I have now added three more; *viz.* the vitriolic, the nitrous, and the vegetable. The vitriolic acid air is produced by boiling in oil of vitriol any inflammable matter, or almoſt any thing that contains *phlogiſton*; as oil, camphor, ſpirit of wine, charcoal, and moſt of the metals. For though this acid ſeems to have no affinity with ſome of theſe ſubſtances when it is cold, it affects them conſiderably, and particularly takes *phlogiſton* from them, when it is hot; and by means of the *phlogiſton*, of which it deprives them, it is early made volatile, ſo as to aſſume the form of a tranſparent air, like that of the marine acid; being as readily imbibed by water, and as readily forming a white cloud upon the admiſſion of alkaline air. But the affinities of the vitriolic acid air with various ſubſtances, and many of the phænomena attending it, are ſtrikingly different from thoſe of the marine acid air. I thought it a little ſingular, that the ſolution of iron, zinc, and tin, in a diluted vitriolic acid ſhould yield inflammable air; and that when boiled in the ſame acid concentrated, they ſhould chiefly yield acid air, which is not at all inflammable, and cannot be confined by water. This, however, is in fact the produce of the proceſs; and the very ſame as when copper, ſilver, or quickſilver is boiled in the ſame acid. From gold, platina, or lead, I was not able to procure any air at all by this means.

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The vegetable acid air is as eaſily procured from the concentrated vegetable acid, as the marine acid air is from ſpirit of ſalt; and, I think, in greater quantity. This air alſo is perfectly tranſparent, is inſtantly imbibed by water, and makes a white cloud upon the admiſſion of alkaline air; though ſeveral of its properties are exceedingly different from thoſe of the marine or vitriolic acid airs.

The nitrous acid I have exhibited in the form of air, though only, as it were, for a moment; ſince no fluid, that I am acquainted with, is capable of confining it. The more I conſider the nitrous acid, the more wonderful and inexhauſtible the ſubject appears. The kinds of air which it forms, according to its various combinations with *phlogiſton*, are, I believe, more numerous than all the kinds that can be formed by the other acids. Many of the phænomena which have lately occurred to my obſervation relating to it are, to me, altogether inexplicable; though I perceive certain analogies among ſome of them. Upon this ſubject I ſhall have a pretty long chapter. But, to avoid being tedious at preſent, I ſhall only obſerve, that by boiling various hard ſubſtances containing *phlogiſton*, and eſpecially charcoal, in the nitrous acid, I get genuine nitrous air, the very ſame that I get from the ſolution of various metals in that acid. At the time of my laſt publication I had not a large burning lens; and as the focus of the mirror cannot be thrown upon any thing in the form of a powder, or that requires a ſolid ſupport, my experiments with the ſolar rays were exceedingly incomplete. I have now procured one of twelve inches in diameter; and the uſe of it has more than anſwered my higheſt expectations. The manner in which I have uſed it, has been to throw the focus upon the ſeveral ſubſtances I wiſhed to examine, either *in vacuo*, or when confined by quickſilver, in veſſels filled with that fluid, and ſtanding with their mouths immerſed in it. I preſently found that different ſubſtances yield very different kinds of air by this treatment; and though the reaſons, or analogies, of the different products, in many of the caſes, be ſufficiently obvious, and ſuch as I had conjectured *à priori*, yet in other caſes I am not a little puzzled and ſurprized. Various metals yield inflammable air by this proceſs; ſeveral ſaline ſubſtances yield fixed air; many metallic *calces* yield the ſame, and ſome of them a *phlogiſticated* common air; and ſome of the precipitates, in which the nitrous acid was employed, yield nitrous air, in one or other of its forms. But the moſt remarkable of all the kinds of air that I have produced by this proceſs is, one that is five or ſix times better than common air, for the purpoſe of reſpiration, inflammation, and, I believe, every other uſe of common atmoſpherical air. As I think I have ſufficiently proved, that the fitneſs of air for reſpiration depends upon its capacity to receive the *phlogiſton* exhaled from the lungs, this ſpecies may not improperly be called, *dephlogiſticated air*. This ſpecies of air I firſt produced from *mercurius calcinatus per ſe*, then from the red precipitate of mercury, and now from red lead. The two former of the ſubſtances yield it pure; but the red lead I have generally met with yields a greater proportion of fixed air along with it. Another quantity, however, gave this air and hardly any thing elſe. On what this difference depends I cannot tell; but hope to be able to inveſtigate. That this air is of that exalted nature, I firſt found by means of nitrous air, which I conſtantly apply as a teſt of the fitneſs of any kind of air for reſpiration, and which I believe to be a moſt accurate and infallible teſt for that purpoſe. Applying this teſt, I found, to my great ſurprize, that a quantity of this air required about five times as much nitrous air to ſaturate it, as common air requires. Common air is diminiſhed about one-fifth, by a mixture of one-half nitrous air; but one quantity of this air was diminiſhed one-half, and another two-thirds, by the addition of twice as much nitrous air; and three times the quantity, left it little more than it was at the firſt. A candle burned in this air with an amazing ſtrength of flame; and a bit of red hot wood crackled and burned with a prodigious rapidity, exhibiting an appearance ſomething like that of iron glowing with a white heat, and throwing out ſparks in all directions. But to complete the proof of the ſuperior quality of this air, I introduced a mouſe into it; and in a quantity in which, had it been in common air, it would have died in about a quarter of an hour, it lived, at two different times, a whole hour, and was taken out quite vigorous; and the remaining air appeared to be ſtill, by the teſt of nitrous air, as good as common air. This experiment I alſo repeated,

peated, and with nearly the ſame ſucceſs, with another mouſe, and another quantity of this air, the virtue of which had been impaired. Examining all the degrees of the calcination of lead, I found nothing but fixed air, or a little *phlogiſticated* common air, till I came to *maſticot*, which is a ſtate that precedes the red lead. This gave air about twice as good as common air, and the litharge, which follows the red lead, gave fixed again. Roman vitriol and ſedative ſalt yielded air which was, as nearly as poſſible, of the ſame degree of purity with common air. My conjectures concerning the cauſe of theſe appearances are as yet too crude to lay before the Society. My preſent ideas of the laſt mentioned facts are, that, together with other obſervations which I ſhall lay before the publick, they afford ſome foundation for ſuppoſing, that the nitrous acid is the baſis of common air, and that nitre is formed by a decompoſition of the atmoſphere. But it is poſſible I may think otherwiſe to-morrow. It is happy, when with a fertility of invention ſufficient to raiſe *hypotheſes*, a perſon is not apt to acquire too great attachment to them. By this means they lead to the diſcovery of new facts, and from a ſufficient number of theſe the true theory of nature will eaſily reſult.

I have made many other original experiments of a miſcellaneous nature; but I ſhall not take up your time with the mention of them in this place. If this imperfect communication gives you, or the Society, any ſatiſfaction, I ſhall be very happy, and ſhall be encouraged to proſecute theſe inquiries as much as my leiſure from other purſuits will admit. I am, &c.

LETTER II.

TO THE REV. DR. PRICE, F. R. S.

DEAR SIR, April 1, 1775.

AS you are pleaſed to intereſt yourſelf in my experiments, I hope it will give you ſome pleaſure to be informed, that, I think, I never was more ſucceſsful than I have been in the few days that I have been able to attend to theſe matters, ſince my return into the country. By the heat of the flame of a candle, and catching the air that ariſes in the manner deſcribed in fig. VIII. pl. 2. in my late Treatiſe, I get the pure air I diſcovered in London in great plenty, from a variety of cheap materials; not only from red lead, but many earthy ſubſtances moiſtened with ſpirit of nitre and dried, as chalk and quick-lime; which demonſtrates that red lead, *mercurius calcinatus per ſe*, &c. extract the nitrous acid from the air; and that this acid is the moſt eſſential among the various ingredients which compoſe the atmoſphere. From tobacco-pipe clay, and ſome other things, moiſtened with ſpirit of nitre, I get fixed air; which ſeems to prove that this ſpecies of air (which is a kind of acid) is a modification of the nitrous acid, and in ſome meaſure accounts for the exiſtence of ſo much fixed air in the atmoſphere. I believe this experiment is the firſt inſtance of the proper generation of fixed air from other principles. What we have got of it hitherto has been by diſlodging it from ſubſtances, that were ſuppoſed to contain it. Notwithſtanding red lead yields ſo pure an air, paint made with it diminiſhes common air, and makes it noxious, as much as white paint; which ſeems to prove, that it is the oil, that yields the *phlogiſton*, which injures the air to which it is expoſed.

L E T T E R III.

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR, London, May 25, 1775.

AS I am deſirous to preſent to the Royal Society a general review of my late obſervations on air, without troubling them with a detail of my experiments, I beg you would lay before them the following particulars, in addition to thoſe contained in the letter which I took the liberty to write to you, dated March 15, and in the extract from that to Dr. PRICE, dated April 1, 1775; ſubmitting the whole to the diſpoſal of the Society.

I have found, that the earths of all denominations, even the cryſtalline and the talcky, which are thought to be inſoluble in acids, yield a pure *dephlogiſticated air*, when treated in the manner mentioned in my former letters; but that the calcareous earths, and ſome of the earths of metals, as red lead and the flowers of zinc, yield it in the greateſt plenty. Upon the whole, I think, it may ſafely be concluded, that the pureſt air is that which contains the leaſt *phlogiſton*: that air is impure (by which I mean that it is unfit for reſpiration, and for the purpoſe of ſupporting flame) in proportion as it contains more of that principle; and that there is a regular gradation from *dephlogiſticated air*, through common air, and *phlogiſticated air*, down to nitrous air; the laſt ſpecies of air containing the moſt, and the firſt-mentioned the leaſt *phlogiſton* poſſible, the common baſis of them all being the nitrous acid; ſo that all theſe kinds of air differ chiefly in the quantity of *phlogiſton* they contain; though with reſpect to nitrous air, there ſeems to be a farther difference in the mode of combination. By attending to the quantity of *phlogiſton* contained in the ſubſtances with which the ſpirit of nitre is mixed, any of theſe kinds of air may be produced at pleaſure, and ſometimes all the kinds will be produced in the different ſtages of the ſame proceſs. White wood-aſhes yield an exceedingly pure air; but the leaſt bit of charcoal in the aſhes depraves the air; and if there be much charcoal in them, the whole produce will be ſtrongly nitrous. The phænomena of detonation (which has been a very puzzling appearance in chemiſtry) admit of a very eaſy explication by the help of my late experiments. It is generally ſuppoſed, that in this caſe a ſulphur is formed, by the union of the nitrous acid and the *phlogiſton* of the body with which it is detonated; which ſulphur is ſo inflammable, that it cannot exiſt a moment without decompoſition: and it has been thought, that in the proceſs of making the *clyſſus* of nitre, the acid is intirely deſtroyed or changed. But, in both theſe caſes, I have no doubt, that the acid enters into the compoſition of ſome of the kinds of air which are generated upon thoſe occaſions. I once mixed a quantity of the ore of lead with ſpirit of nitre, and when it was dry, put it into a gun-barrel, filled up to the mouth with ſand, in order to collect the air that heat would expel from it, in the uſual manner. The production of air was very great and rapid; and when the heat became conſiderable, all the contents of the gun-barrel were exploded with great violence, and a loud report, demoliſhing the veſſel which I had placed to receive the air. The next time, putting the ſame materials into a glaſs veſſel, and diſpoſing the apparatus in ſuch a manner as that the exploſion could not affect the collected air, I found it to be very ſtrongly nitrous. Such, therefore, I conclude to be the produce of the exploſion of gun-powder, ſince charcoal with ſpirit of nitre yields this kind of air. In the detonation of nitre with ſubſtances that contain little *phlogiſton*, the acid may form common air, or air much purer than that.

As I mean theſe letters to contain a general outline of what I have lately obſerved with reſpect to air, I ſhall add, that by the favour of that moſt intelligent and generous chemiſt Mr. WOLFE, I have lately procured ſome of that phoſphoric ſpar, from which a new mineral acid, firſt diſcovered in Sweden, is procured. This acid, as well as the marine, vitriolic, and vegetable, I throw into the form of air, confined by quickſilver; and thus have an opportunity of examining its affinities with the greateſt eaſe and certainty. I ſhall in this place only obſerve with reſpect to it, that this acid air decompoſes nitre, but not near ſo rapidly as the marine acid air; and that the ſalt which is formed by its union with alkaline air is not ſenſibly ſoluble in water. I am, &c.

P. S. Upon ſecond thoughts, I am not ſo well ſatisfied with my conjecture, hinted in the letter to Dr. PRICE, that fixed air may be a modification of the nitrous acid, though the experiment there mentioned ſeems to make it probable. But I lay no ſtreſs upon any opinions, farther than as they may lead to the diſcovery of new facts.

XXXIX. *An Account of the* Gymnotus Electricus.

By John Hunter, *F. R. S.*

Redde, May 11, 1775.

TO Mr. WALSH, the firſt diſcoverer of animal electricity, the learned will be indebted for whatever the following pages may contain, either curious or uſeful. The ſpecimen of the animal which they deſcribe was procured by that Gentleman; and at his requeſt this diſſection was performed, and this account of it is communicated.

This fiſh, on the firſt view, appears very much like an eel, from which reſemblance it has moſt probably got its name; but it has none of the ſpecific properties of that fiſh. This animal may be conſidered, both anatomically and phyſiologically, as divided into two parts; *viz.* the common animal part; and a part which is ſuperadded, *viz.* the *peculiar organ.* I ſhall at preſent conſider it only with reſpect to the laſt; as the firſt explains nothing relating to the other, nor any thing relating to the animal œconomy of fiſh in general. The firſt, or common animal part, is ſo contrived as to exceed what was neceſſary for itſelf, in order to give ſituation, nouriſhment, and moſt probably the peculiar property to the ſecond. The laſt part, or peculiar organ, has an immediate connexion with the firſt; the body affording it a ſituation; the heart, nouriſhment; and the brain, nerves and probably its peculiar powers. For the firſt of theſe purpoſes, the body is extended out in length, being much longer than would be ſufficient for what may be called its progreſſive motion. For the real body, or that part where the *viſcera* and parts of generation lye, is ſituated, with reſpect to the head, as in other fiſh, and is extremely ſhort; ſo that, according to the ordinary proportions, this ſhould be a very ſhort fiſh. Its great length, therefore, ſeems chiefly intended to afford a ſurface for the ſupport of the peculiar organ: however, the tail-part is likewiſe adapted to the progreſſive motion of the whole, and to preſerve the ſpecific gravity; for the ſpine, *medulla ſpinalis*, muſcles, fin, and air-bladder, are continued through its whole length. Beſides which parts, there is a membrane paſſing from the ſpine to that fin which runs along the belly or lower edge of the animal. This membrane is broad at the end next to the head, terminating in a point at the tail. It is a ſupport for the abdominal fin, gives a greater ſurface of ſupport for the organ, and makes a partition between the organs of the two oppoſite ſides.

THE ORGANS.

The organs which produce the peculiar effect of this fiſh, conſtitute nearly one-half of that part of the fleſh in which they are placed, and perhaps make more than one-third of the whole animal. There are two pair of theſe organs, a larger, and a ſmaller; one being placed on each ſide. The large pair occupy the whole lower or anterior, and alſo the lateral part of the body, making the thickneſs of the fore or lower parts of the animal, and run almoſt through its whole length; *viz.* from the *abdomen* to near the end of the tail [(a)]. It is broadeſt on the ſides of the fiſh at the anterior end, where it incloſes more of the lateral parts of the body, becomes narrower towards the end of the tail, occupying leſs and leſs of the ſides of the animal, till at laſt it ends almoſt in a point. Theſe two organs are ſeparated from one another at the upper part, by the muſcles of the back, which keep their poſterior or upper edges at a conſiderable diſtance from one another(b); below that, and towards the middle, they are ſeparated by the air-bag(c); and at their lower parts they are ſeparated by the middle partition(d). They begin forwards, by a pretty regular edge, almoſt at right angles with the longitudinal axis of the body, ſituated on the lower and lateral parts of the *abdomen*. Their upper edge is a pretty ſtraight line, with ſmall indentations made by the nerves and blood veſſels, which paſs round it to the ſkin. At the anterior end they go as far towards the back as the middle line of the animal; but in their approach towards the tail they gradually leave that line, coming nearer to the lower ſurface of the animal. The general ſhape of the organ, on an external or ſide view, is broad at the end next to the head of the animal, becoming gradually narrower towards the tail, and ending there almoſt in a point. The other ſurfaces of the organ are fitted to the ſhape of the parts with which they come in contact; therefore on the upper and inner ſurface it is hollowed, to receive the muſcles of the back(e). There is alſo a longitudinal depreſſion on its lower edge, where a ſubſtance lies; which divides it from the ſmall organ, and which gives a kind of fixed point for the lateral muſcles of the fin(f). Its moſt internal ſurface is a plane adapted to the partition which divides the two organs from one another(g). The edge next to the muſcles of the back is very thin, but the organ becomes thicker and thicker towards its middle, where it approaches the center of the animal. It becomes thinner again, towards the lower ſurface or belly; but that edge is not ſo thin as the other(h). Its union with the parts to which it is attached is in general by a looſe, but pretty ſtrong, cellular membrane; except at the partition, to which it is joined ſo cloſe as to be almoſt inſeparable.

(a) Vide pl. II. fig. 1. KKK.

(b) Pl. IV. cccc. (c) Pl. IV. D. (d) Pl. IV. K. (e) Pl. IV.

The ſmall organ lies along the lower edge of the animal, nearly to the ſame extent as the other(i). Its ſituation is marked externally by the muſcles which move the fin under which it lies. Its anterior end begins nearly in the ſame line with the large organ, and juſt where the fin begins. It terminates almoſt inſenſibly near the end of the tail, where the large organ alſo terminates. It is of a triangular figure, adapting itſelf to the part in which it lies(k). Its anterior end is the narroweſt part; towards the tail it becomes broader; in the middle of the organ it is thickeſt; and from thence becomes gradually thinner to the tail, where it is very thin. The two ſmall organs are ſeparated from one another by the middle muſcles, and by the bones upon which the bones of the fins are articulated (l). The large and the ſmall organ on each ſide, are ſeparated from one another by a membrane, the inner edge of which is attached to the middle partition, and its outer edge is loſt on the ſkin of the animal (m). To expoſe the large organ to view, nothing more is neceſſary than to remove the ſkin, which adheres to it by a looſe cellular membrane. But to expoſe the ſmall organ, it is neceſſary to remove the long row of ſmall muſcles which move the fin.

(f) Pl. IV. (g) Pl. IV. (h) Pl. IV. (i) Pl. II. fig. 1. LLL.
(k) Pl. IV. II.

OF THE STRUCTURE OF THESE ORGANS.

The ſtructure is extremely ſimple and regular, conſiſting of two parts; *viz.* flat partitions or *ſepta*, and croſs diviſions between them. The outer edge of theſe *ſepta* appear externally in parallel lines nearly in the direction of the longitudinal axis of the body (n). Theſe *ſepta* are thin membranes, placed nearly parallel to one another. Their lengths are nearly in the direction of the long axis, and their breadth is nearly the ſemi-diameter of the body of the animal (o). They are of different lengths, ſome being as long as the whole organ. I ſhall deſcribe them as beginning principally at the anterior end of the organ, although a few begin along the upper edge; and the whole, paſſing towards the tail, gradually terminate on the lower ſurface of the organ; the lowermoſt, at their origin terminating ſooneſt. Their breadths differ in different parts of the organ. They are in general broadeſt near the anterior end, anſwering to the thickeſt part of the organ, and become gradually narrower towards the tail; however, they are very narrow at their beginnings or anterior ends. Thoſe neareſt to the muſcles of the back are the broadeſt, owing to their curved or oblique ſituation upon theſe muſcles, and grow gradually narrower towards the lower part, which is in a great meaſure owing to their becoming more tranſverſe, and alſo to the organ becoming thinner at that place(p). They have an outer and an inner edge. The outer is attached to the ſkin of the animal, to the lateral muſcles of the fin, and to the membrane which divides the great organ from the ſmall; and the whole of their inner edges are fixed to the middle partition formerly deſcribed, alſo to the air-bladder, and three or four terminate on that ſurface which incloſe the muſcles of the back(q). Theſe *ſepta* are at the greateſt diſtance from one another at their exterior edges near the ſkin, to which they are united; and as they paſs from the ſkin towards their inner attachments they approach one another(r). Sometimes we find two uniting into one. On that ſide next to the muſcles of the back, they are hollow from edge to edge, anſwering to the ſhape of thoſe muſcles; but become leſs and leſs ſo towards the middle of the organ; and from that towards the lower part of the organ, they become curved in the other direction(s). At the anterior part of the large organ, where it is nearly of an equal breadth, they run pretty parallel to one another, and alſo pretty ſtreight;

(l) Pl. III. E.

(m) Pl. III. P.

(n) Pl. II. KKK.

(o) Pl. IV. HH.

(p) Pl. IV. where the different breadths are ſeen in one view.

(q) Pl. IV. (r) Pl. IV. (s) Pl. IV.

but where the organ becomes narrower, it may be obſerved in ſome places, that two join or unite into one; eſpecially where a nerve paſſes acroſs. The termination of this organ at the tail is ſo very ſmall that I could not determine, whether it conſiſted of one *ſeptum* or more. The diſtances between theſe *ſepta* will differ in fiſhes of different ſizes. In a fiſh of two feet four inches in length, I found them $\frac{1}{27}$ of an inch diſtant from one another; and the breadth of the whole organ, at the broadeſt part, about an inch and a quarter, in which ſpace were thirty-four *ſepta*. The ſmall organ has the ſame kind of *ſepta*, in length paſſing from end to end of the organ, and in breadth paſſing quite acroſs; they run ſomewhat ſerpentine, not exactly in ſtraight lines(t). Their outer edges terminate on the outer ſurface of the organ, which is in contact with the inner ſurface of the external muſcle of the fin, and their inner edges are in contact with the centre-muſcles. They differ very much in breadth from one another; the broadeſt being equal to one ſide of the triangle, and the narroweſt ſcarcely broader than the point or edge. They are pretty nearly at equal diſtances from one another; but much nearer than thoſe of the large organ, being only about $\frac{1}{56}$th part of an inch aſunder: but they are at a greater diſtance from one another towards the tail, in proportion to the increaſe of breadth of the organ. The organ is about half an inch in breadth, and has fourteen *ſepta*. Theſe *ſepta*, in both organs, are very tender in conſiſtence, being eaſily torn. They appear to anſwer the ſame purpoſe with the columns in the *torpedo*, making walls or butments for the ſub-diviſions, and are to be conſidered as making ſo many diſtinct organs. Theſe *ſepta* are interſected tranſverſely by very thin plates or membranes, whoſe breadth is the diſtance between any two *ſepta*, and therefore of different breadths in different parts; broadeſt at that edge which is next to the ſkin; narroweſt at that next to the center of the body, or to the middle partition which divides the two organs from one another. Their lengths are equal to the breadths of the *ſepta*, between which they are ſituated. There is a regular ſeries of them continued from one end of any two *ſepta* to the other. They appear to be ſo cloſe as even to touch. In an inch in length there are about 240, which multiplies the ſurface in the whole to a vaſt extent.

(t) Pl. II. LLL.

OF THE NERVES.

The nerves in this animal may be divided into two kinds; the firſt, appropriated to the general purpoſes of life; the ſecond, for the management of this peculiar function, and very probably for its exiſtence. They ariſe in general from the brain and *medulla ſpinalis*, as in other fiſh; but thoſe from the *medulla* are much larger than in fiſh of equal ſize, and larger than is neceſſary for the common operations of life. The nerve which ariſes from the brain, and paſſes down the whole length of the animal (which I believe exiſts in all fiſh) is larger in this than in others of the ſame ſize, and paſſes nearer to the ſpine(u). In the common eel it runs in the muſcles of the back, about midway between the ſkin and ſpine. In the cod it paſſes immediately under the ſkin. From its being larger in this fiſh than in others of the ſame ſize, one might ſuſpect, that it was intended for ſupplying the organ in ſome degree; but this ſeems not to be the caſe, as I was not able to trace any nerves going from it to join thoſe from the *medulla ſpinalis*, which run to the organ. This nerve is as ſingular an appearance as any in this claſs of animals; for ſurely it muſt appear extraordinary, that a nerve ſhould ariſe from the brain to be loſt in common parts, while there is a *medulla ſpinalis* giving nerves to the ſame parts. It muſt ſtill remain one of the inexplicable circumſtances of the nervous ſyſtem. The organ is ſupplied with nerves from the *medulla ſpinalis* from which they come out in pairs between all the *vertebræ* of the ſpine(x). In their paſſage from the ſpine they give nerves to the muſcles of the back, &c. They bend forwards and outwards upon the ſpine, between it and the muſcles, and ſend out ſmall nerves to the external ſurface, which join the ſkin near to the lateral lines. Theſe ramify upon the ſkin, but are principally bent forwards between it and the organ, into which they ſend ſmall branches as they paſs along. They ſeem to be loſt in theſe two parts. The trunks get upon the air-bladder, or rather dip between it and the muſcles of the back, and continuing

(u) Pl. III. T. (x) Pl. III. s.

their courſe forwards upon that bag, they dip in between it and the organ, where they divide into ſmaller branches; then they get upon the middle partition, on which they continue to divide into ſtill ſmaller branches; after which they paſs on, and get upon the ſmall bones and muſcles, which are the baſes for the under fin, and at laſt they are loſt on that fin. After having got between the organ and the above mentioned parts, they are conſtantly ſending ſmall nerves into the organs; firſt into the great organ, and then into the ſmall one; alſo into the muſcles, of the fin, and at laſt into the fin itſelf. Theſe branches, which are ſent into the organ as the trunk paſſes along, are ſo ſmall, that I could not trace their ramifications in the organs. In this fiſh, as well as in the *torpedo*, the nerves which ſupply the organ are much larger than thoſe beſtowed on any other part for the purpoſes of ſenſation and action; but it appears to me, that the organ of the *torpedo* is ſupplied with much the largeſt proportion. If all the nerves which go to it were united together, they would make a vaſtly greater chord, than all thoſe which go to the organ of this eel. Perhaps when experiments have been made upon this fiſh, equally accurate with thoſe made upon the *torpedo*, the reaſon for this difference may be aſſigned.

BLOOD VESSELS.

How far this organ is vaſcular, I cannot poſitively determine; but from the quantities of ſmall arteries going to it, I am inclined to believe, that it is not deficient in veſſels. The arteries ariſe from the large artery which paſſes down the ſpine; they go off in ſmall branches like the *intercoſtals* in the human ſubject, paſs round the air-bladder, and get upon the partition together with the nerves, and diſtribute their branches in the ſame manner. The veins take the ſame courſe backwards, and enter the large vein which runs parallel with the artery.

PLATE I. FIG. I.

Shews the whole animal of the full ſize. It lies on one ſide; which poſture expoſes the whole of the under fin. The head is twiſted, to ſhew its upper part, on which are ſeen the eyes, &c.

FIG. II.

Shews the animal lying in the ſame poſition, but the head is twiſted in the contrary direction, ſo as to expoſe its under ſurface. Between the two fins, and before the beginning of the under fin, is the cavity of the belly of the fiſh; at the anterior part of which cavity is the *anus*.

PLATE II. FIG. I.

Exhibits the whole of the two organs on each ſide, the ſkin being removed as far as theſe organs extend. A. The lower ſurface of the head of the animal. B. The cavity of the belly. C. The *anus*. D. The fin. E. The back of the fiſh where the ſkin has not been removed. FF. The fin which runs along the lower edge of the fiſh. GGG. The ſkin turned back. HHH. The lateral muſcles of the above fin removed and carried back with the ſkin, to expoſe the ſmall organ. I. Part of the muſcle left in its place. KKK. The large organ. LLL. The ſmall organ. MMMM. The ſubſtance which divides the large organ from the ſmall. N. At this place the above ſubſtance is removed.

PLATE III. FIG. IV.

A ſection of the whole thickneſs of the fiſh near the upper part, a little magnified. The ſkin is removed as far back as the poſterior edge of the organ, and the other parts immediately belonging to it, ſuch as the *medulla ſpinalis*. There are ſeveral pieces or ſections taken out of the organ, which expoſe every thing that has any relation to it. At the upper and lower ends of the figure, F F, the organ is entire, the ſkin only being removed. AA. The body of the animal near the back, covered by the ſkin. BB. The belly-fin, covered alſo by the ſkin. C. Part of the ſkin removed from the organ, and turned back. DD. The muſcles which move the fin laterally, and which immediately cover the ſmall organ. E. The middle muſcles of the fin, which lay immediately between the two ſmall organs. FF. The outer ſurface of the large organ, as it appears when the ſkin is removed. G. The ſmall organ, as it appears when the lateral muſcles are removed. HH. The cut ends of the muſcles of the back, which have been removed to expoſe the deeper

Plate 4

Plate 3

Fig 5

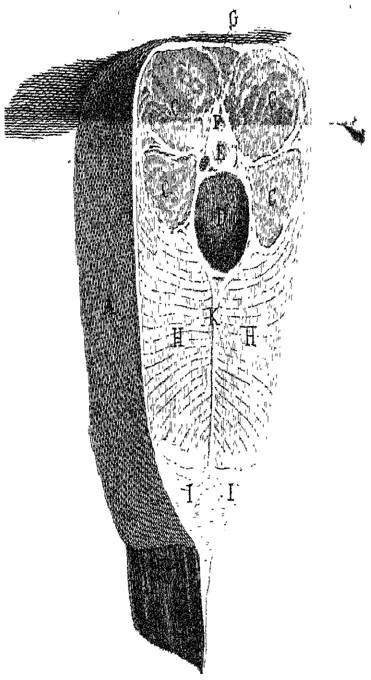
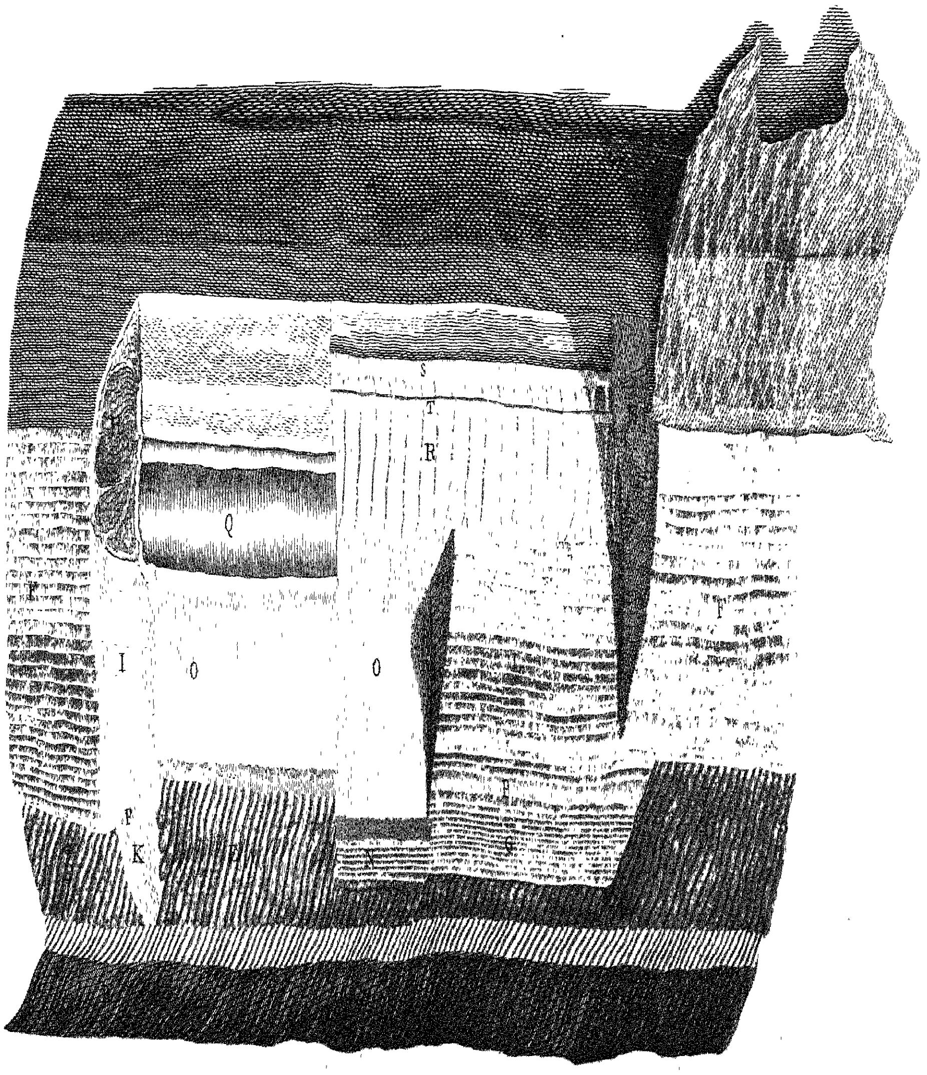


Fig 4



James Roberts delin et sculp

ſeated parts. II. The cut ends of the large organ, part of which has alſo been removed, to expoſe the deeper ſeated parts. K. The cut end of the ſmall organ. L. A part of the large organ, the reſt having been removed. M. The cut end of the above ſection. N. A ſection of the ſmall organ. OO. The middle partition which divides the two large organs. P. A fatty membrane, which divides the large organ from the ſmall. Q. The air-bladder. R. The nerves going to the organ. S. The *medulla ſpinalis*. T. The ſingular nerve.

PLATE IV. FIG. V.

A tranſverſe ſection of the fiſh, expoſing at one view, all the parts of which it is compoſed. A. The external ſurface of the ſide of the fiſh. B. The under fin. CCCC. The cut ends of the muſcles of the back. D. The cavity of the air-bladder. E. The body of the ſpine. F. The *medulla ſpinalis*. G. The large artery and vein. HH. The cut ends of the two large organs. II. The cut ends of the two ſmall organs, K. The partition between the organs.

XL. *Obſer-*

XL. *Some Obſervations upon* Myrrh, *made in* Abyſſinia, *in the Year* 1771, *and ſent to* William Hunter, *M. D. with Specimens, in* February, 1775. *By* James Bruce, *Eſq.*

Redde, June 1, 1775.

THE ancients, and particularly DIOSCORIDES, have ſpoken of myrrh in ſuch a manner, as to leave us no alternative, but to ſuppoſe either that they have deſcribed a drug which they had never ſeen; or, that the drug ſeen and deſcribed by them is abſolutely unknown to modern naturaliſts and phyſicians. The Arabs, however, who form the link of the chain between the Greek phyſicians and ours, in whoſe country the myrrh was produced, and whoſe language gave it its name, have left us undeniable evidence, that what we know by the name of myrrh, is in nothing different from the myrrh of the ancients, growing in the ſame countries from which it was brought formerly to Greece; that is, from the Eaſt coaſt of Arabia Felix, bordering on the Indian Ocean, and that low land in Abyſſinia on the South-eaſt of the Red-ſea, included nearly between the 12th and 13th degree of North latitude, and limited on the Weſt by a meridian paſſing through the iſland Maſſowa; and on the Eaſt by another, paſſing through Cape Guardſoy, without the ſtraits of Bab el Mandel. This country the Greeks knew by the name of the Troglodytria; not to be confounded with another nation of Troglodytes, very different in all reſpects, living in the foreſts between Abyſſinia and Nubia. The myrrh of the Troglodytes was always preferred to that of Arabia; and it has maintained this preference to our days. That part of Abyſſinia being half over-run and ſettled, half waſted and abandoned, by a barbarous nation from the Southward, very little correſpondence or commerce has been ſince carried on between the Arabians and that coaſt; unleſs by ſome deſperate adventures of Mahometan merchants, made under favourable and accidental circumſtances, which have ſometimes ſucceeded, and very often likewiſe have miſcarried. The moſt frequent way by which this Troglodyte myrrh is exported, is from Maſſowa, a ſmall Abyſſinian iſland, on the coaſt of the Red-ſea. But notwithſtanding this, the quantity of Abyſſinian myrrh is ſo very ſmall, in compariſon of that of Arabia ſent to Grand Cairo, that we may ſafely attribute to this only the reaſon, why our myrrh is not ſo good in quality as the myrrh of the ancients, which was Abyſſinian. Although thoſe barbarians make uſe of the gum, leaves, and bark, of this tree, in diſeaſes to which they are ſubject; yet, as very little is wanted for ſuch purpoſes, and the tree is the common timber of the country, this does not hinder them from cutting it down every day, to burn for the common uſes of life; and as they never plant, or replace the trees deſtroyed, it is probable, that in ſome years the true Troglodyte myrrh will not exiſt; and the erroneous deſcriptions of the Greek phyſicians will lead poſterity, as they have done us now, into various conjectures, all of them falſe, upon the queſtion, what that myrrh of the ancients was?

Though the myrrh of the Troglodytes was ſuperior to any Arabian, yet the Greeks perceived, that it was not all of equal goodneſs. PLINY and THEOPHRASTUS make this difference to ariſe from the trees being partly wild, partly cultivated. But this is an imaginary reaſon; all the trees were wild. But it was the age of the tree and its health, the manner of making the cut or wound in it, the time of gathering the myrrh, and the circumſtances of the climate when it was gathered, that conſtantly determined, and does yet determine, the quality of the drug. In order to have myrrh of the firſt, or moſt perfect ſort, the ſavages chuſe a young, vigorous tree, whoſe bark is without moſs, or any paraſite-plant; and, above the firſt large branches, give the tree a deep wound with an axe. The myrrh which flows, the firſt year, through this wound, is myrrh of the firſt growth; and never in very great quantity. This operation is performed ſome time after the rains have ceaſed; that is, from April to June; and the myrrh is produced in July and Auguſt. The ſap once accuſtomed to iſſue through this gaſh, continues ſo to do ſpontaneouſly, at the return of every ſeaſon: but the tropical rains, which are very violent, and continue ſix months, waſh ſo much dirt, and lodge ſo much water in the cut, that in the ſecond year, the tree has begun to rot and turn foul in that part, and the myrrh is of a ſecond quality, and ſells in Cairo about a third cheaper than the firſt. The myrrh alſo produced from gaſhes near the roots, and in the trunks of old trees, is of the ſecond growth and quality, and ſometimes worſe. This, however, is the *good* myrrh of the Italian ſhops every where but in Venice. It is of a blackiſh red, foul colour, ſolid and heavy, loſing little of its weight by being long kept; and it is not eaſily diſtinguiſhed from that of Arabia Felix. The third and worſt kind is gathered from old wounds or gaſhes, formerly made, in old trees; or myrrh that, paſſing unnoticed, has hung upon the tree ungathered a whole year; black and earth-like in colour, and heavy, with little ſmell and bitterneſs. This apparently is the *caucalis* of the ancients.

PLINY ſpeaks of *ſtacte*, as if it was freſh or liquid myrrh; and DIOSCORIDES, in his chapter upon it (cap. 67.), ſays ſomething like this alſo. However, it is not credible, that the ancients, either Greeks or Latins, placed at ſuch a diſtance, could ever ſee the myrrh in that ſtate. The natives of its country ſay, that it hardens on the tree inſtantly, on being expoſed to air; and I, who was ſeveral months within four days journey of the place where it grew, and had the ſavages quite at my devotion to go and come from thence, could never ſee the neweſt myrrh ſofter than the ſtate it now is in; though, I think, it diſſolved more perfectly in water, than when it had been kept. DIOSCORIDES too mentions a kind of myrrh which, he ſays, was green, and of the conſiſtence of paſte. But as SERAPION and the Arabs ſay, that *ſtacte* was a preparation of myrrh diſſolved in water, it is probable, that this unknown green kind of DIOSCORIDES was, like the *ſtacte*, a compoſition of myrrh and ſome other ingredient, not a ſpecies of Abyſſinian myrrh, which he could never have ſeen, either ſoft or green.

It may be remarked, that when we buy freſh or new myrrh, it has always a very ſtrong, rancid, oily ſmell; and when thrown into water, *globules* of an oily matter ſwim upon the ſurface. This greaſineſs is not from the myrrh; it is owing to the ſavages uſing goats-ſkins anointed with butter (to make them ſupple) wherein to put their myrrh at gathering; and in theſe ſkins it remains, and is brought to market: ſo that, far from its being a fault, as ſome ignorant druggiſts at Rome and Venice believe, it is a mark that the myrrh is freſh gathered, which is the beſt quality that myrrh of the firſt ſort can have. Beſides, far from hurting the myrrh, this oily covering muſt rather at firſt have been of ſervice; as it certainly impriſons and confines the volatile parts of new myrrh, which eſcape in great quantities, to a very conſiderable diminution in the weight. The piece of myrrh which I ſend you, is what a fine tree, leſs than fifteen inches diameter in the trunk at the bottom, wounded in two places, produced at one of the wounds, in the year 1771. And it may be regarded as the only unexceptionable and authentic evidence, in Europe, of what the Troglodyte myrrh was; unleſs it be thoſe pieces ſtill remaining in my collection, and a piece, ſomewhat ſmaller than yours, which I gave to the king of

France's cabinet at Paris. This piece which I ſend you, had loſt near ſix dragmes Troy of its weight, between the 27th of Auguſt, 1771, and the 29th of June, 1773. It has loſt a very few grains ſince. It was kept, as were all the other pieces, with great care in cotton, ſeparately in a box, to prevent its loſing weight by friction.

OPOCALPASUM.

At the time when I was on the borders of the Tal-Tal, or Troglodyte country, I ſought to procure myſelf branches and bark of the myrrh tree, enough preſerved to be able to draw it; but the length and ruggedneſs of the way, the heat of the weather, and the careleſsneſs and want of reſources of naked ſavages, always diſappointed me. In thoſe goat-ſkin bags into which I had often ordered them to put ſmall branches, I always found the leaves moſtly in powder; ſome few that were intire, ſeemed to reſemble much the *acacia vera*, but were wider towards the extremity, and more pointed immediately at the end. In what order the leaves grew, I never could determine. The bark was abſolutely like that of the *acacia vera*; and among the leaves I often met with a ſmall ſtraight weak thorn, about two inches long. Theſe were all the circumſtances I could combine, relative to the myrrh tree, too vague and uncertain to riſk a drawing upon, when there ſtill remained ſo many *deſiderata* concerning it; and as the king was obſtinate not to let me go thither, after what had happened to the ſurgeon, mate, and boat's crew, of the Elgin Indiaman, I was obliged to abandon the drawing of the myrrh tree to ſome more fortunate traveller. At the ſame time that I was taking theſe pains about the myrrh, I had deſired the ſavages to bring me all the gums they could find, with the branches and bark of the trees that produced them. They brought me, at different times, ſome very fine pieces of incenſe, and at another time, a very ſmall quantity of a bright colourleſs gum, ſweeter on burning than incenſe; but no branches of either tree, though I found this latter afterwards, in another part of Abyſſinia. But at all times they brought me quantities of gum, of an even and cloſe grain, and of a dark-brown colour, which was produced by a tree called *ſaſſa*: and twice I received branches of this tree in tolerable order; and of theſe I made a drawing. Some weeks after, walking in a Mahometan village, I ſaw a large tree, with the whole upper part of the trunk and the large branches ſo covered with great boſſes and knobs of gum, as to appear monſtrous: and aſking farther about the tree, I found that it had been brought, many years before, from the myrrh country by merchants, and planted there for the ſake of its gum, with which theſe Mahometans ſtiffened the blue Surat cloths, which they got damaged from Mocha, to trade in with the Galla and Abyſſinians. Neither the tree which they called *ſaſſa*, nor the name, nor the gum, could allow me to doubt a moment that it was the ſame as what had been brought to me from the myrrh country; but I had the additional ſatisfaction to find the tree all covered over with beautiful crimſon flowers, of a very extraordinary and ſtrange conſtruction. I began then a drawing anew, with all that ſatisfaction known only to thoſe who have been converſant in ſuch diſcoveries. I took pieces of the gum with me. It is very light. GALEN complains, that in his time, the myrrh was often mixed with a drug which he calls *opocalpaſum*, by a Greek name; but what this drug was, is totally unknown to us at this day. But, as the only view of the ſavage, in mixing another gum with his myrrh, muſt have been to increaſe the quantity, and as the great plenty, in which this gum is produced, and its colour make it very proper for this uſe; and above all, as there is no reaſon to think, there is another gum-bearing tree of equal qualities in the country where the myrrh grows, it ſeems to me next to a proof, that this muſt have been the *opocalpaſum*. I muſt, however, confeſs, that GALEN ſays, the *opocalpaſum* was ſo far from an innocent drug, that it was a mortal poiſon, and had produced very fatal effects. But as thoſe Troglodytes, though now more ignorant than formerly, are ſtill well acquainted with the properties of their herbs and trees, it is not poſſible, that the ſavage, deſiring to increaſe his ſales, would mix them with a poiſon that muſt needs diminiſh them. And we may therefore, without ſcruple, ſuppoſe, that GALEN was miſtaken in the quality aſcribed to this drug; and that he might have imagined, that people died of the *opocalpaſum*, who perhaps really died of the phyſician. Firſt, becauſe we know of no gum or *reſin* that is a mortal poiſon: ſecondly, becauſe, from the conſtruction of its parts, gum is very ill adapted for having the activity which violent poiſon has; and conſidering the ſmall quantities in which myrrh is taken, and the *opocalpaſum* could have been but in an inconſiderable proportion to the myrrh, to have killed, it muſt have been a very active poiſon. Thirdly, theſe accidents, from a known cauſe, muſt have brought myrrh into diſuſe, as certainly as the Spaniards mixing *arſenic* with the bark, would baniſh that drug when we ſaw people die of it. Now this never was the caſe: it maintained its character among the Greeks and the Arabs, and ſo down to our days; and a modern phyſician thinks it might make man immortal, if it could be rendered perfectly ſoluble in the human body. GALEN then was miſtaken as to the poiſonous quality of the *opocalpaſum*. The Greek phyſicians knew little of the natural hiſtory of Arabia; leſs ſtill of that of Abyſſinia; and we who have followed them know nothing of either. This gum being put into water ſwells and turns white, and loſes all its glue; it reſembles gum *adragant* much in quality, and may be eaten ſafely. This ſpecimen came from the Troglodyte country in the year 1771: a piece of myrrh from Arabia Felix, and a piece of gum of the *ſaſſa* from Abyſſinia were packed up in another ſeparate box to be ſent you for compariſon, but forgotten by my ſervant. They will be ſent hereafter. The *ſaſſa*, the tree which produces the *opocalpaſum*, does not grow in Arabia. Arabian myrrh is eaſily known from Abyſſinian by the following method:

take a handful of the ſmalleſt pieces, found at the bottom of the baſket where the myrrh was packed, and throw them into a plate, and juſt cover them with water a little warm; the myrrh will remain for ſome time without viſible alteration, for it diſſolves ſlowly; but the gum will ſwell to five times its original ſize, and appear ſo many white ſpots amidſt the myrrh.

The pieces ſent you are, N° 1. Virgin Troglodyte myrrh. N° 2. The worſt ſort of Troglodyte myrrh, called *cancabs*. N° 3. *Opocalpaſum* from the myrrh-country.

XLI. *An Account of a curious Giant's Cauſeway, or Group of angular Columns, newly diſcovered in the* Euganean Hills, *near* Padua, *in* Italy. *In a Letter from* John Strange, *Eſq. F.R.S. to Sir* John Pringle, *Bart. P.R.S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR, Venice, March 10, 1775.

Redde, June 15, 1775.

AS you thought proper, to recommend to the notice of our learned Society the account, which I did myſelf the honour of communicating to you not long ſince, of two giants cauſeways in this Venetian ſtate; I now take the liberty to ſend you the deſcription and figure of another ſimilar phænomenon, equally curious, and lately diſcovered in the ſame neighbourhood. It is ſituated at Caſtel Nuovo, a ſmall village near Teolo, alſo in the Euganean hills, about four miles South-weſt of the other Giant's Cauſeway of Monte Roſſo before deſcribed. I am indebted for the intelligence of this new cauſeway to the ingenious Abbé FORTIS, whom curioſity alſo led among thoſe hills; and who, at my requeſt, accompanied a painter I lately ſent from hence, to make the drawing of it, which I have now the pleaſure to tranſmit to you(a). *Il Saſſo di San Biaſio,* which is the name of the ſpot where this cauſeway is ſituated, is a large inſulated rock, compoſed of the ſame ſort of grey granite that is common to the Euganean hills, and which I have before deſcribed(b). The columns which form this cauſeway, partly againſt the flank of the rock, and partly round its baſe, are of the ſame ſubſtance, with the rock itſelf, to which they adhere, as I have conſtantly obſerved in all ſimilar groups. They are therefore of a compound nature, like the columns of Monte Roſſo, and differ intirely from the common ſort, which are moſtly homogeneous, or of an uniform texture; as is obſervable in the jointed, as well as ſimple ſpecies of *baſaltes*. I ſhall take the firſt opportunity of ſending a fragment of one of the newly diſcovered columns, for the inſpection of the Society; in the mean time the incloſed pieces, which were broken from one of them, will ſerve to ſhew, how different their ſubſtances is from that of the common baſaltic columns. By comparing theſe pieces with the fragments of the columns of Monte Roſſo which I before tranſmitted to the Society, ſome eſſential difference will appear between them. Thoſe of San Biaſio, though very hard, are rather porous, of a lighter colour than the columns of Monte Roſſo, and very much reſemble a ſpecies of *lava*, which I have often ſeen. This porouſneſs I alſo remember to have once before obſerved, and more ſignally too, in ſome baſaltic columns near Achon, in the province of Auvergne, in France. The pores in the columns of both theſe groups are alſo irregularly diſperſed, and of unequal ſize, like thoſe of pumice ſtones and other common *pori ignei*. Thoſe of the columns of San Biaſio are moreover commonly inveſted with a ſort of *crocus martis*, which I have alſo frequently obſerved in the pores of other vulcanic concretions. Theſe properties are ſurely further marks in favour of the igneous origin of ſuch columnar cryſtallizations; eſpecially, ſince they ſeem contrary to the principle by which the common aqueous cryſtals are formed, ſucceſſively, *et per juxtapoſitionem partium ad partes*. In fact, theſe cryſtals manifeſt no ſuch poroſity. I alſo obſerved, that the columns of Achon, though of a homogeneous ſubſtance, yet differ from the common *baſaltes* by their immenſe ſize as well as colour, which is rather brown than black. The columns of San Biaſio are likewiſe very large, meaſuring often two feet in diameter. They are alſo of the ſimple ſpecies, that is not jointed, and moſtly quadrangular, which figure ſeems rather a principal characteriſtic of this group, being rarely obſerved in others. So true it is, as I formerly remarked, that ſome particular characteriſtic ever diſtinguiſhes the different groups of *baſaltes*; which, therefore, cannot be too narrowly obſerved, before we pretend to form any opinion about their origin. Some few, but very few, chiefly of the ſmaller columns of San Biaſio, are of a pentagonal form, like the ſpecimen which I propoſe to tranſmit to the Society. But there are no hexagonal columns, which, in other baſaltic groups, are the moſt common. The natural poſition of theſe columns, whether facing the rock, or about the bottom of it, is moſtly perpendicular. Another adjacent portion of this rock is alſo characterized by angular and, as it were, winding *ſtrata*, ſomewhat reſembling the bending pillars of Staffa, as may be obſerved in the drawing. The rock itſelf is alſo compoſed of angular maſſes, as are indeed moſt granites; and theſe maſſes are alſo ranged perpendicularly. Several emerge, as it were, from the tops and ſides of the neighbouring rocks and hills, like ſo many ſtately and artificial pillars. The winding *ſtrata* before mentioned are alſo parallel with each other, as I have frequently obſerved in other granites, as well as common vulcanic *ſtrata* in general, particularly of the harder ſort. DESMAREST calls the latter *Baſaltes en tables*(c); which is a kind of vulcanic ſlate, formed in parallel *ſtrata* of different thickneſs, from two or three to five and ſix inches. This is very common in the provinces of Velay and Auvergne, in France, where it is alſo uſed for coverings of houſes. The ſame ſort of ſlate is likewiſe common to the mountains of Genoa, many of which ſeem to be of vulcanic origin, as I recollect to have remarked in paſſing the chain of the *bocbetta*, between Genoa and the plain of Lombardy. I mention this circumſtance, as the vulcanic *phænomena* of that part of Italy have not hitherto been attended to. In fact, it is lately only that ſuch obſervations begin to be made in other countries; the characters of extinct volcanos or vulcanic tracts, being but little known, though ſuch tracts ſeem to occupy every where a very conſiderable part of the ſurface of the earth. I remember to have obſerved theſe ſlaty tables, or parallel *ſtrata*, of granite, near the top of the famous San Gothard, in the aſcent of that mountain on the ſide towards Switzerland. Theſe *ſtrata* are alſo ranged perpendicularly, like the other common ones in granites, and reſemble DESMAREST'S *baſaltes en tables*; affording thus another proof of the analogy remarkable between the organization of the different maſſes in granites, and that of common vulcanic *ſtrata* in general. The former, as well as the latter, have their priſmatic columns, their *baſaltes en tables*, as DESMAREST calls them, and *en boules*, as I have obſerved in my account of Monte Roſſo. Surely, therefore, theſe are ſtrong proofs in favour of the common origin of both. The rocks of San Biaſio abound with ferruginous vitrifications, which are frequently obſervable in granites; and the neighbouring tracts with *lava* or *pori ignei*; as I have alſo obſerved, when I made the tour of this country, particularly about Teolo. The Abbé FORTIS brought me a piece of *lapis lenticularis*, broken from the limeſtone that ſuperficially covers the granite of theſe Euganean hills, in many places, as I before obſerved. I mention this circumſtance, recollecting to have taken notice, in my laſt paper, that ſuch figured bodies are not commonly found in the lime-ſtone of this country. As the preſent account may ſerve, by way of appendix, to that which I lately did myſelf the honour to

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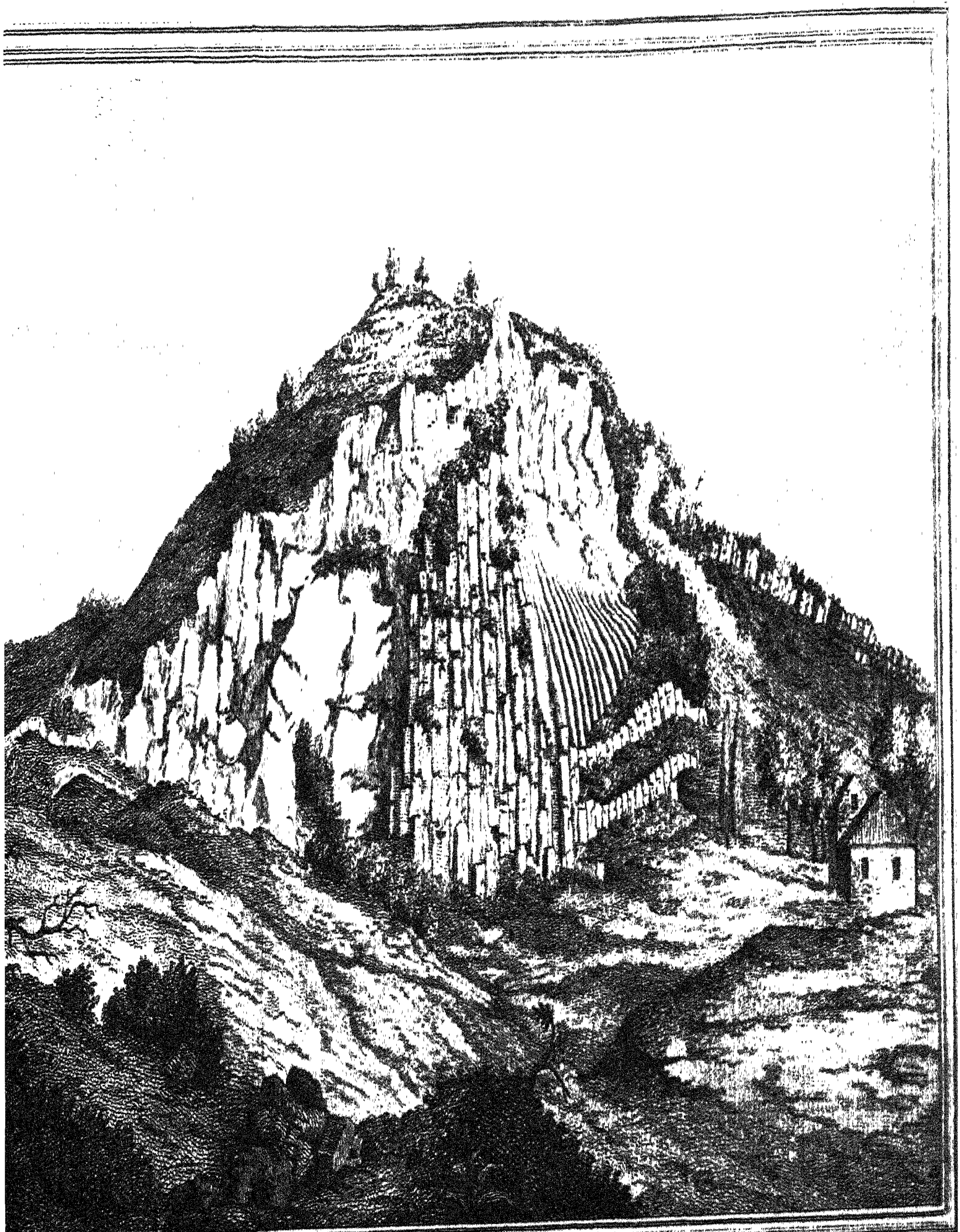
(a) Plate XI.

(b) See Article II. of this volume.

(c) Encyclopedie, Art. Pavé des Giants.



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preſent to the Society, through your favour; you will oblige me alſo by the communication of it to that learned body, ſhould you find it deſerving of their attention.

I have nothing further to add at preſent, but the aſſurances of my being, with great truth and eſteem, SIR,

Your moſt obedient humble ſervant,

JOHN STRANGE.

XLII. *Obſervations on the Difference between the Duration of Human Life in Towns and in Country Pariſhes and Villages. By the Rev.* Richard Price, *D. D. F. R. S. Communicated by Dr.* Horſley.

Redde, June, 22, 1775.

THIS Society has lately been much obliged to Dr. PERCIVAL, for the accounts he has communicated of the ſtate of population at Mancheſter and its adjacent places. Theſe accounts contain ſome facts, which appear to me curious and important. From the laſt in particular, there appears to be reaſon for concluding, that whereas a 28th part of the inhabitants die annually in the town of Mancheſter, not more than a 56th part die annually in the adjacent country. This implies a difference ſo great between the rates of human mortality in theſe different ſituations, that ſome, whoſe judgements I reverence, have thought it incredible. I will, therefore, beg leave to offer the following obſervations on this ſubject.

In the firſt place, the evidence in this inſtance is ſuch as ſeems to leave little room for doubt. From an accurate ſurvey it appears, that the number of inhabitants in the town was 27246, in the year 1773. The number of deaths the ſame year (and alſo the average for 1772, 1773, and 1774), was 973[(a)]; that is, a 28th part of the number of inhabitants. From an equally careful ſurvey it appears, that the number of inhabitants in that part of the pariſh of Mancheſter which lies in the country, was 13786. The number of deaths in 1772 was 246; that is, a 56th part of the number of inhabitants. The chief objection to this evidence is, that the number of deaths in that part of the pariſh which lies in the country is given only for one year; whereas the average of ſeveral years ought to be given. But firſt, the number of deaths in 1772, in the town, was nearly the ſame with the medium for ſeven years; and from hence there ariſes a probability, that in the adjacent country, the number of deaths, in the ſame year, could not have been much lower than the medium. Secondly, ſuppoſing it lower, there is the higheſt probability, that it was not more than a 4th or 5th lower. Suppoſe then the true annual medium to be 300, inſtead of 246, and it will follow, that whereas a 28th part of the inhabitants die in the town annually, a 46th part die in the country; and this is a difference very conſiderable. But farther, I would obſerve, that the difference which this ſurvey gives between the rate of mortality in the town of Mancheſter and the adjacent country, is confirmed by a variety of other accounts. It may be ſtated in general, that whereas in great towns, the proportion of inhabitants dying annually is from 1 in 19 to 1 in 22 or 23, and in moderate towns from 1 in 24 to 1 in 28 (b); in country pariſhes and villages on the contrary, this proportion ſeldom exceeds 1 in 40 or 50. The proofs of this are numerous and unexceptionable; and I have elſewhere given a particular account of them (c). I will here only mention the following facts.

(a) The numbers of burials in the town, including the addition of 50 every year for diſſenters, was in 1772, 954
1773, 973
1774, 1008

Within the pariſh, but out of the town, there are 13 epiſcopal and diſſenting chapels; and the number of burials in all theſe chapels, in 1772, was 246. The chriſtenings were 401. The number of burials brought from the country into the town is not conſiderable; and it is, I am informed, pretty exactly balanced by the burials carried out of the town into the country.

The number of inhabitants at STOCKHOLM in 1763 was 72979. The average of deaths for the ſix preceding years had been 3802 (d). One, therefore, in nineteen died there annually.

(b) The number dying annually in towns is ſeldom ſo low as 1 in 28, except in conſequence of a rapid increaſe produced by an influx of people, at thoſe periods of life when the feweſt die. This is the caſe at Mancheſter. It is alſo the caſe at Liverpool and at Berlin; in the former of which towns, 1 in 27 dies annually; and in the latter, 1 in 26½ died from 1755 to 1759. See Obſervations on Reverſionary Payments, p. 224, &c. 3d edition.

(c) See Obſervations on Reverſionary Payments, &c. Eſſay 1ſt, and Supplement.

(d) See a Memoir by M. WARGENTIN, in the 15th volume of the *Collection Academique*, printed at Paris, 1772. From this memoir I learn, that in 1757, and 1760, and 1763, a ſurvey was made of the inhabitants of Sweden, diſtinguiſhing, particularly, the numbers of both ſexes living at every age; and that alſo, for nine years (or from 1755 to 1763), an exact regiſter was kept of the number of births and burials in each year, diſtinguiſhing the age and ſex of every one that died. I do not know whether this regulation has been continued

At ROME, an account is taken every year of the number of inhabitants; and, in the year 1771, it was 159675. The average of deaths for ten years had been 7367: one, therefore, in 21½ died annually.

In LONDON I have ſhewn, with an evidence which I think little ſhort of demonſtration, that at leaſt 1 in 20¾ of the inhabitants die annually(c). And, from a particular ſurvey and a very accurate regiſter of mortality at NORTHAMPTON, it appears, that 1 in 26½ die there annually.

Let theſe facts be compared with the following. In 1767, a ſurvey was made of the inhabitants of the iſland of MADEIRA, under the direction of Dr. THOMAS HEBERDEN, and their number was found to be 64614. The average of burials for eight preceding years had been 1293. Only 1 in 50, therefore, of the inhabitants died annually (ſee Philoſophical Tranſactions, vol. LVII. p. 461.).

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to the preſent time; but the reſult of it, for the nine years I have mentioned, as given by M. WARGENTIN in this Memoir, contains indeed a moſt curious account of the ſtate of population in Sweden; and it is particularly to my preſent purpoſe to mention, that it ſhews, that though a 19th part of the inhabitants of Stockholm die every year, yet in the whole kingdom, taking all the towns and country together, not more than a 35th part die every year. In 1757, Sweden conſiſted of 1101595 males and 1221600 females; in 1760, of 1121053 males and 1246445 females; and in 1763, of 1165489 males and 1280905 females. The annual average of births, from 1755 to 1763, was 46223 males and 44017 females; of marriages, 21219; of deaths, 34088 males and 35037 females.

(c) See Obſervations on Reverſionary Payments, Eſſay IV. p. 253, &c.

The diſtrict of VAUD, in Switzerland, in 1766, contained 112951 inhabitants. The average of deaths for ten preceding years had been 2504. Only 1 in 45, therefore, died annually (f).

The number of inhabitants in the pariſh of ACKWORTH, in the county of York, in 1757, was 603; and the average of deaths for ten years had been $10\frac{7}{10}$, or a 56th part. In 1767, the inhabitants were increaſed to 728; and the annual average of deaths was $15\frac{3}{10}$, or nearly a 47th part (g).

The reaſon of this ſtriking difference between the rate of human mortality in towns and in country pariſhes and villages muſt be, firſt, the luxury and the irregular modes of life which prevail in towns; and, ſecondly, the foulneſs of the air. But it has been inquired, whether the migrations of people from the country to towns may not produce this difference, by leſſening the proportion of inhabitants that die in the country, and increaſing the ſame proportion in towns? In anſwer to this enquiry I would obſerve: firſt, that this difference being a difference of near a half, it is apparently much greater than can be accounted for by any ſuch cauſe. But, ſecondly, it ſhould be conſidered, that if migrations leſſen the number of deaths, they alſo leſſen the number of inhabitants; and that it depends intirely on the ages at which the inhabitants remove from a place, whether the effect of their removal ſhall be lowering or raiſing the proportion of the annual deaths to the number of inhabitants. In the preſent caſe, the truth appears to be, that the moſt common age of migration from the country is ſuch as raiſes this proportion in the country. This will be evident from the following conſiderations. The period of life in which perſons remove from the country to ſettle in towns, is chiefly the beginning of mature life, or from the age of 10 or 15 to 25 or 30. In infancy, none migrate; and in the decline of life, it is more uſual to retire from towns than to remove to them. Towns, therefore, will be inhabited more by people in the firmeſt parts of life; and, on the other hand, the country will be inhabited more by people in the weakeſt parts of life; and the conſequence of this is, that in the country, the inhabitants muſt die faſter in proportion to their number than they otherwiſe would, and that in towns they muſt die more ſlowly. In particular, the number of children is always much greater in the country than in towns; and this is a circumſtance which muſt be extremely unfavourable to the former: for it is well known, that there are no years of life, in which ſo many of a given number die, as the firſt three or four years. Till the age of five, human life, like a fire beginning to burn, is very feeble; and in ſome ſituations more than half, and in others, a third or fourth of all that are born die before that age. After this, life grows leſs and leſs precarious till it acquires its utmoſt vigour at 10 or 15; and of the living at this age, not above 1 in 70 or 80 dies annually in the worſt ſituations; and in the beſt ſituations, not above 1 in 150 or 160. After 15, life declines, and continues to do ſo more and more, till it becomes quite extinct in old age. If, therefore, in any ſituation, the inhabitants conſiſt more of perſons in mature life, and yet die faſter, it muſt be owing to ſome particular cauſes of mortality that operate there. This is the caſe in all towns where any obſervations have been made. Mancheſter, in particular, is not only kept up, but increaſes faſt, by removals to it of perſons in the prime of life. The country round it increaſes likewiſe; but it is by an exceſs of the births above the deaths; that is, by acceſſions to it of children in the very feebleſt part of life. This ought to raiſe the proportion of annual deaths to inhabitants in the country, much above the ſame proportion in the town; but, inſtead of this, it is near one-half lower.

(f) See M. MURET's Memoir on the State of Population, in the *Pays de Vaud*, printed at Bern, in 1766; and the Supplement to the Obſervations on Reverſionary Payments, p. 358. 3d edit.

(g) I owe this information concerning the pariſh of Ackworth to a curious regiſter kept there by Dr. LEE. I have taken the liberty to inſert this regiſter in the poſtſcript, together with the annual regiſter and ſurvey of Rome from 1762 to 1771.

It may be needleſs to add any thing to theſe obſervations.

In order, however, to put this matter out of all doubt, I will obſerve farther, that it appears in fact, from the accounts furniſhed by Dr. PERCIVAL, that the number of inhabitants in the periods of life when mankind die faſteſt(n) (that is, in the firſt and laſt ſtages of life), is conſiderably leſs in the town of Mancheſter than in the adjacent country. The number of inhabitants in the town, under 15 and above 50, is 13467; in the country, 7305. And the whole number is, in the town, 27246; in the country, 13786. In the town, therefore, the inhabitants, in the firſt and laſt ſtages of life, do not make half the whole number; but in the country, they make conſiderably more than half. At Ackworth, likewiſe, in Yorkſhire, the inhabitants under 15 and above 50 are more than half the whole number; and the ſame is true at Hale near Altringham, at Horwich, at Darwen near Blackburn in Lancaſhire, and at Cockey Moor(i) near

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(n) In towns, about a fourth of the inhabitants die commonly between 14 and 51; a fifth or ſixth die at 51 and upwards; and the remainder die under 15. In country pariſhes and villages about a fifth die between 14 and 51; about two-fifths at 51 and upwards; and the remainder under 15.

(i) I am much indebted to Dr. PERCIVAL for the following account of theſe places. The ſociety belonging to the chapel at Hale is compoſed of 140 males, 136 females, 92 married perſons, 8 widowers, 12 widows, 105 under 15, and 41 above 50. The deaths, during ſeven years, have been 28, and the births 68. Mr. EVANS's congregation at Horwich, conſiſts of 305 individuals; *viz.* 149 males, 156 females, 94 married perſons, 9 widowers, 8 widows, 127 under 15 years of age, and 50 above 50. The births, for ſeven years, 101; the deaths 32. A 66th part, therefore, die annually in both theſe places. The rev. Mr. SMALLEY's congregation at Darwent, conſiſts of 1850 individuals; *viz.* 900 males, 950 females, 640 married perſons, 30 widowers, 48 widows, 737 perſons under the age of 15, and 218 above 50. During the laſt ſeven years the births have amounted to 508, the deaths to 233. A 56th part, therefore, die annually. Mr. BARNES's congregation at Cockey Moor, conſiſts of 154 families and 711 individuals; namely, 320 males, 391 females, 248 married perſons, 10 widowers, 27 widows, 252 perſons under the age of 15, and 99 above 50. Deaths in ſeven years 114; in which period the deaths were conſiderably increaſed by an uncommon fatality of the ſmall-pox. One perſon in 44 died annually. The rev. Mr. MERCER's congregation at Chowbent, in Lancaſhire, conſiſts of 1160 perſons; *viz.* 554 males, 606 females, 173 males and 150 females under the age of ten, 83

Bolton, in the ſame county; and yet in ſome of theſe places it appears, that not a 60th part of the inhabitants die annually.

At Stockholm, in 1763, the inhabitants under the age of 5, were only a 12th; above 70, only a 46th part of the whole number. But in all Sweden, the number under 5 was a 7th; and above 70, near the 32d part of all the inhabitants: and yet 35 die in the town to 19 in the whole kingdom. This may be eaſily deduced from Mr. WARGENTIN's tables in the *Collection Academique* before quoted.

To the accounts which give the proportion of inhabitants to annual deaths ſo high as 50 or 60 to 1, it has been farther objected, that if true, it muſt follow, that in ſuch ſituations half the inhabitants muſt live to 50 or 60 years of age. But were this a right inference, there would be nothing in it incredible. For though in moſt cities one-half die in the firſt two or three years after birth; yet, in many country ſituations, the greater part live to marry: and in the pariſh of Ackworth, particularly, it appears with undeniable evidence from the regiſter, that one-half of all born there live to the age of 46. It appears alſo, with equal evidence, from M. MURET's tables in the Bern Memoirs for 1766, that in 43 pariſhes in the diſtrict of Vaud, one-half of all born there

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males and 91 females above 50, 398 married perſons, 26 widowers, and 43 widows. The baptiſms during ſix years, wanting ſix weeks, have amounted to 293, and the deaths to 169. One perſon, therefore, in 41 died annually. Theſe ſurveys were made in the year 1773.

live beyond the age of 41. In truth, did all mankind lead natural and virtuous lives, that waſte of the ſpecies which happens in infancy and childhood would not take place, and few would die except in old age. The inference, however, which I have mentioned, cannot be made with reaſon. It is juſt only in the particular caſe of an uniform decreaſe in the probabilities of living from birth to old age; and this is a caſe that has never exiſted. In all other caſes, there is not any neceſſary connexion between the proportion of inhabitants dying annually, and the age to which the greater part live. In moſt cities one-half, as I have juſt obſerved, of all that are born die before two or three years of age. But it cannot be imagined, that there is any place where ſo many as one-half or a third of the inhabitants die every year.

But to return to Dr. PERCIVAL's account of the town and pariſh of Mancheſter. It appears from this account, that the number of children under 15, compared with the number of inhabitants between 14 and 51, is greater in the country than in the town of Mancheſter, in the proportion of no leſs than 5 to 4(k). It follows, therefore, that though, in conſequence of a conſtant influx of people to the town, it is more filled than the country with inhabitants in the moſt vigorous periods of life; yet one child in four leſs is born in the town than in the country. This is a remarkable circumſtance, and the reaſons of it muſt be the two following. Firſt, the town inhabitants being leſs healthy, and dying faſter, have not the ſame ſtrength of conſtitution with the country inhabitants. Secondly, in the town a ſmaller proportion of the adult inhabitants marry; and they marry later than in the country. The ſurvey fully proves this; for it appears, that though the number of inhabitants at the moſt common marrying ages, compared with the whole number of the living above the age of 14, is ſmaller in the country than the town; yet the proportion of the married to the living above 14, is very nearly the ſame in both ſituations. And there are more widows and widowers in the town than in the country in the proportion of near 16 to 11. We learn from hence, I think, clearly in what manner towns operate in checking population, and preventing the increaſe of mankind.

(k) In the town, the number of inhabitants between 14 and 51 is 13779; and 9575 under 15. In the country, the former number is 6481; and the latter, 5545. But the laſt number would have been only 4503, had the proportion of the inhabitants between 14 and 51 to the inhabitants under 15 been the ſame in both ſituations. It is owing to this, that the number of perſons in a family in the country is 5½; but in the town only 4¼.

Dr. PERCIVAL informs us, that the reverend and learned Dr. TUCKER has been led, by ſome obſervations he has made at Briſtol, to doubt whether the common opinion is right, with reſpect to the diſproportion between the number of male and female births; and that he, therefore, wiſhes a farther inquiry may be made into this ſubject. This has induced me to collect the following facts, which, I think, will abundantly ſettle this point.

In

	Born Males.	Females.	Proportion.
In London for the laſt 110 years, or from 1664 to 1773,	862293	817072	20 to 19
Paris, for 8 years *(l)*,	79693	76481	25 to 24
Leyden, for 50 years *(m)*,	46773	44933	26 to 25
Vienna, for 27 years, ending 1746 *(n)*,	67060	64893	31 to 30
Berlin, for 40 years, ending 1761 *(o)*,	71188	67431	20 to 19
Kurmark of Brandenburgh, for 9 years, ending 1759 *(p)*,	102425	96521	18 to 17
Dukedom of Magdeburgh, for 38 years, ending 1759 *(q)*,	153227	145985	21 to 20
All the Pruſſian towns, for a courſe of years, *(r)*,	691826	659072	21 to 20
In a great number of country pariſhes, for a courſe of years *(s)*,	59067	56282	21 to 20
In the ſame country pariſhes, for another period of years *(t)*,	89530	84954	19 to 18
Leeds, Mancheſter, Coventry, &c. for a period of years *(u)*,	108784	103449	20 to 19
In the ſame towns, for another period *(x)*,	57084	54128	20 to 19
Total,	2388950	2271201	20 to 19
Sweden, for 9 years, ending 1763,	416007	396124	20 to 19

Mr. DERHAM, in his Phyſico-Theology, p. 175. has ſtated the proportion of male to female births at 14 to 13, and this proportion has ever ſince been generally received as the true one; but it appears from this table, that it ought to have been ſtated at 20 to 19. But though it appears, that the number of males born is in this proportion greater than the number of females born, yet, in moſt places, the number of males living has been

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(l) See SUSM. GOTTLICKE Ordnung Tables, p. 16. *(m)* Ibid. p. 17. *(n)* Ibid. p. 13. *(o)* Ibid. p. 12. *(p)* Ibid. p. 3. *(q)* Ibid. p. 5. *(r)* Ibid. p. 9. *(s)* See Dr. SHORT's New Obſervations, p. 27. 31. *(t)* Ibid. p. 30. *(u)* Ibid. p. 49. *(x)* Ibid. p. .

found to be leſs than the number of females. The reaſon is, without doubt, that males are more ſhort-lived than females; and this is owing partly to the peculiar hazards to which males are ſubject, and their more irregular modes of life; but it is owing principally to ſome particular delicacy in the male conſtitution, which renders it leſs durable: for there are many obſervations which prove, that the greater mortality of males takes place chiefly in the firſt and laſt ſtages of life. A few facts of this kind I will beg leave to mention, becauſe I have juſt met with them.

In the pariſh of St. Sulpice, at Paris, during 30 years, 5 males under a year old died to 4 females. But under 10, only 13 males died to 12 females (ſee SUSMILCH. Tables, vol. II. p. 30.)

In Stockholm, during 9 years ending in 1763, the number of ſtill-borns amounted to 666; of whom 390 were males, and 276 females; that is, 10 to 7. The number of the living in the town above the age of 80 was, in 1760, 332; of whom 248 were females, and 84 males, or near 3 to 1. In the whole kingdom of Sweden, including all town and country inhabitants, the number of ſtill-borns, during the 9 years juſt mentioned, was 19845; of whom 11424 were males, and 8421 females, or near 4 to 3. The number of the living in the whole kingdom conſiſted of more females than males, in the proportion of 10 to 9. It conſiſted of more females turned of 80 than males, in the proportion of 33 to 19; and of more females turned of 90 than males in the proportion of near 2 to 1. (See M. WARGENTIN's Memoir in the *Collection Academique*, vol. XV.) Having now had occaſion to refer again to this Memoir, I will juſt add, that it appears, that by the exceſs of the births above the deaths, Sweden gains every year an addition of above 20000 inhabitants; and that in ſix years they increaſed from 2323195 to 2446394. I am afraid, were regulations eſtabliſhed for a ſimilar inquiry in this kingdom, we ſhould be far from finding our ſtate ſo encouraging. London alone is a gulph which ſwallows up an increaſe equal to near three-fourths of that of Sweden.

POSTSCRIPT.

The following tables have been ſelected from ſeveral more of the ſame kind in M. WARGENTIN's Memoir on the ſtate of population in Sweden. I have inſerted them here, becauſe they fully verify moſt of the obſervations in the preceding paper, and contain more diſtinct and authentic information on the ſubject of human mortality than I have ever before met with.

TABLE I.

Shewing the order of human mortality in SWEDEN.

	Annual deaths, being the average of three years, 1761, 1762, and 1763.			Number of the living in 1763.	
	Males.	Females.		Males.	Females.
Still-born,	1324	988	Born,	47216	44892
Died under 1	11172	9850	Living under 1	36094	35453
Died between 1 and 3	4393	4336	Living betw. 1 and 3	66059	67234
3——5	2206	2249	3——5	66454	67711
5——10	2151	2057	5——10	130019	130758
10——15	933	834	10——15	126696	128021
15——20	711	658	15——20	108312	109985
20——25	834	756	20——25	92299	105115
25——30	883	863	25——30	88056	101003
30——35	1020	1146	30——35	85936	95811
35——40	955	923	35——40	74826	814[illegible]3
40——45	1180	1170	40——45	67448	74854
45——50	1099	938	45——50	52398	59551
50——55	1280	1113	50——55	47298	56646
55——60	1177	1097	55——60	37086	45537
60——65	1586	1721	60——65	34892	44925
65——70	1237	1566	65——70	20649	28964
70——75	1322	2041	70——75	15454	23159
75——80	1092	1695	75——80	8858	13556
80——85	917	1446	80——85	4620	7487
85——90	414	650	85——90	1508	2694
Above 90	215	379	Above 90	527	988
Total of annual deaths,	36777	37488	Total of the living at all ages,	1165489	1280905

In this table it is obſervable, that the number of the living, in every equal diviſion of life from birth, decreaſes continually till all become extinct; and that though the males born are more than the females born, in the proportion of 20 to 19; yet the males living of all ages are leſs in number, in the proportion of 1165489 to 1280905, or nearly of 10 to 11; notwithſtanding which, the males that die annually are to the females as 52 to 53.

TABLE II.

Shewing the order of human mortality at STOCKHOLM.

	Annual deaths, being the average of three years, 1761, 1762, and 1763.		Number of the living in 1763.		
	Males.	Females.		Males.	Females.
Still-born,	54	43	Born,	1406	1340
Died under 1	567	489	Living under 1	684	733
Died between 1 and 3	161	170	Living betw. 1 and 3	1173	1348
3—5	80	79	3—5	1022	1106
5—10	71	72	5—10	2630	2774
10—15	49	24	10—15	3151	2918
15—20	53	30	15—20	3018	2865
20—25	91	64	20—25	3070	4056
25—30	121	78	25—30	3380	4251
30—35	141	102	30—35	3705	4234
35—40	118	96	35—40	3019	3288
40—45	140	115	40—45	2846	3130
45—50	101	84	45—50	1775	1984
50—55	105	91	50—55	1581	2129
55—60	61	54	55—60	853	1329
60—65	79	88	60—65	826	1383
65—70	41	54	65—70	370	778
70—75	33	77	70—75	260	574
75—80	28	59	75—80	128	324
80—85	18	45	80—85	58	127
85—90	7	20	85—90	16	51
Above 90	3	11	Above 90	10	22
Total of annual deaths,	2068	1902	Total of the living at all ages,	33575	39404

In this table it may be obſerved, that the number living at every age from birth decreaſes only till five. Between 5 and 10 Stockholm begins to receive recruits from the country, and they come in faſter and faſter till 35; after which age it appears, that more die than come in; and that the living in every ſubſequent period goes on decreaſing continually till the end of life. It is farther obſervable, that this table exhibits a greater difference than the former, between the mortality of males and females.

A compariſon of theſe tables will ſhew a ſtriking contraſt in other reſpects between the ſtate of human mortality in the whole kingdom of Sweden and in its capital. In order to make this more obvious and unexceptionable, I will add the following table, deduced from all M. WARGENTIN's tables taken together.

TABLE III.

	In all SWEDEN for nine years.		In STOCKHOLM for 9 years.	
	Males.	Females.	Males.	Females.
Still-born,	1 in 36	1 in 47	1 in 32	1 in $43\frac{1}{2}$
Died under 1 of all born,	1 in $4\frac{1}{3}$	1 in $4\frac{3}{5}$	1 in $2\frac{1}{3}$	1 in $2\frac{3}{10}$
Died annually of the living betw. 1 and 3	1 in $17\frac{1}{3}$	1 in $17\frac{3}{4}$	1 in 7	1 in $7\frac{1}{5}$
Between 3——5	1 in $34\frac{1}{2}$	1 in 36	1 in $13\frac{1}{3}$	1 in 16
5——10	1 in 71	1 in 76	1 in $34\frac{1}{2}$	1 in 39
10——15	1 in 149	1 in 161	1 in 79	1 in 114
15——20	1 in 149	1 in 164	1 in 59	1 in 99
20——25	1 in 108	1 in 139	1 in 44	1 in 79
25——30	1 in 98	1 in 113	1 in 33	1 in 58
30——35	1 in 85	1 in 84	1 in 31	1 in 43
35——40	1 in 78	1 in 91	1 in $26\frac{1}{3}$	1 in 39
40——45	1 in 56	1 in 63	1 in 23	1 in 31
45——50	1 in 49	1 in 65	1 in $19\frac{1}{2}$	1 in 28
50——55	1 in 37	1 in 50	1 in $16\frac{1}{2}$	1 in $25\frac{1}{2}$
55——60	1 in 31	1 in 40	1 in 14	1 in 24
60——65	1 in 23	1 in 26	1 in 11	1 in 16
65——70	1 in 17	1 in $18\frac{1}{2}$	1 in $9\frac{1}{2}$	1 in $13\frac{1}{3}$
70——75	1 in $11\frac{1}{3}$	1 in $11\frac{1}{2}$	1 in $7\frac{3}{10}$	1 in 8
75——80	1 in 8	1 in $8\frac{1}{3}$	1 in $4\frac{1}{2}$	1 in 5
80——85	1 in $5\frac{1}{4}$	1 in $5\frac{1}{3}$	1 in $3\frac{1}{2}$	1 in $3\frac{1}{3}$
85——90	1 in $3\frac{4}{5}$	1 in 4	1 in 2	1 in $2\frac{1}{3}$
Above 90	1 in $2\frac{1}{2}$	1 in $2\frac{1}{2}$	1 in $2\frac{3}{10}$	1 in $2\frac{1}{3}$
Died of all living at all ages,	1 in $33\frac{1}{2}$	1 in 36	1 in $17\frac{1}{10}$	1 in $21\frac{1}{4}$

A general

A general Bill of all the CHRISTENINGS and BURIALS in the Pariſh of ACKWORTH, in the county of YORK, extracted from the Pariſh Regiſter, for ten Years, from March 25, 1747, to March 25, 1757.

In ten Years Chriſtened, Males 62. Females 65. Total, 127.
In ten Years Buried, Males 58. Females 49. Total, 107.

	Males.	Females	Total.		Males.	Females.	Total.
Whereof have died Under two years old,	6	11	17	And there have died of Apoplexy,	0	1	1
Between 2 and 5	1	2	3	Cancer,	1	0	1
5—10	2	2	4	Cholic,	1	0	1
10—20	1	2	3	Conſumptions,	10	13	23
20—30	6	2	8	Dropſy,	4	1	5
30—40	2	3	5	Fevers,	23	12	35
40—50	11	3	14	Infants,	6	7	13
50—60	9	2	11	Lunacy,	0	1	1
60—70	9	7	16	Old Age,	9	15	24
70—80	9	8	17	Palſey,	1	0	1
80—90	1	6	7	Quinſey,	0	1	1
90—100	1	1	2	Small-Pox,	1	0	1
Of all, in 10 Years,	58	49	107	Of the above Diſtempers, in 10 Y.	56	51	107

In this Pariſh there are { 160 Houſes, twelve of which are uninhabited.
603 Souls of the following Ages; viz.

	Males.	Females.	Total.		Males.	Females.	Total.
Under two years old,	12	19	31	Between 40 and 50	40	22	62
Between 2 and 5	25	19	44	50—60	38	33	71
5—10	30	38	68	60—70	25	14	39
10—20	59	58	117	70—80	4	8	12
20—30	55	41	96	80—90	4	0	4
30—40	26	33	59	90—100	0	0	0
				Total of all Ages,	318	285	603

A general Bill of all the CHRISTENINGS and BURIALS in the Pariſh of ACKWORTH, in the County of YORK, for ten years, from March 25, 1757, to March 25, 1767.

In ten Years Chriſtened, Males, 104. Females, 108. Total, 212.
In ten Years Buried, Males, 79. Females, 77. Total, 156.

	Males.	Females.	Total.
Whereof have died			
Under two years old.	18	13	31
Between 2 and 5	9	7	16
5 —— 10	4	1	5
10 —— 20	2	2	4
20 —— 30	7	5	12
30 —— 40	3	8	11
40 —— 50	2	4	6
50 —— 60	11	3	14
60 —— 70	13	13	26
70 —— 80	7	14	21
80 —— 90	3	6	9
90 —— 100	0	1	1
Of all Ages, in 10 Y.	79	77	156

	Males.	Females.	Total
And there have died of			
Apoplexy,	2	1	3
Aſthma,	2	1	3
Cancer,	0	1	1
Caſualties,	5	1	6
Childbed,	0	2	2
Chincough,	0	2	2
Conſumptions,	23	15	38
Convulſions,	4	2	6
Diabetes,	1	0	1
Dropſy,	0	3	3
Dyſentery,	1	1	2
Fever,	12	11	23
Jaundice,	1	0	1
Infants,	7	6	13
Lunacy,	0	1	1
Meaſles,	0	2	2
Mortification,	2	1	3
Old Age,	11	19	30
Palſey,	1	0	1
Quinſey,	1	0	1
Small-Pox,	7	6	13
Teeth,	0	1	1
Of all the above Diſorders, in 10 Years,	80	76	156

In this Pariſh there are { 184 Houſes, eleven of which are uninhabited.
728 Souls of the following Ages; viz.

	Males.	Females.	Total.
Under two years old,	31	25	56
Between 2 and 5	32	36	68
5 —— 10	34	38	72
10 —— 20	50	51	101
20 —— 30	44	63	107
30 —— 40	61	62	123

	Males.	Females.	Total
Between 40 and 50	31	38	69
50 —— 60	28	32	60
60 —— 70	20	28	48
70 —— 80	7	10	17
80 —— 90	2	4	6
90 —— 100	0	1	1
Total of all Ages,	339	389	728

In 1702 there were only eleven children baptized, ſix of whom are now living in the pariſh, and have reſided here almoſt all the time.

Account of the Inhabitants of Rome, from 1762 to 1771.

	1762	1763	1764	1765	1766	1767	1768	1769	1770	1771
Pariſh Churches,	81	81	81	81	82	82	82	82	82	82
Families,	35739	35696	35453	25771	35894	36375	36409	36521	37449	37285
Biſhops,	42	62	45	45	51	52	54	47	52	62
Prieſts,	2742	2699	2718	2617	2531	2652	2676	2819	3031	2925
Religious of ſundry Orders,	4381	4291	3588	4509	4258	4105	4310	4988	3792	3739
Nuns,	1725	1892	1661	1759	1684	1738	1709	1695	1692	1554
Collegians and ſcholars,	868	970	763	888	734	1153	907	1197	939	491
Cardinals courts or attendants,	812	791	765	544	827	588	491	592	72	665
Poor penſioners of the Hoſpital,	1050	858	1271	1725	1903	2839	2010	1970	1426	1386
Priſoners,	339	240	336	402	370	390	251	405	446	402
Males of all ages,	90239	87396	88618	87205	88280	88577	88865	88415	86610	87547
Females of all ages,	67219	71423	73286	70890	69588	71183	69982	70491	71833	72128
Above 14 years of age,	120696	123211	125391	120300	119661	122150	120820	121455	120385	119984
Under 14,	36762	35608	36508	37795	38207	37610	38027	37451	38058	39691
Nonconformiſts to the church of Rome,	37	61	75	86	120	49	65	77	84	91
Blacks,	9	11	8	8	12	5	10	9	5	5
Devotees,	18	30	28	31	23	22	20	25	20	20
Births,	4989	5336	5420	4828	4962	4310	4595	4891	4967	4216
Deaths,	7149	6493	7361	8375	7722	7528	9574	6972	6646	5850
Total of inhabitants,	157458	158819	161899	158095	157868	159760	158847	158906	158443	159675

XLIII. *Experiments on Animals and Vegetables, with reſpect to the Power of producing Heat.* By John Hunter, *F. R. S.*

Redde, June 22, 1775.

THE ingenious experiments and obſervations lately preſented to this learned Society, upon a power which animals ſeem to poſſeſs of generating cold, induced me to look over my notes of certain experiments and obſervations made in the year 1766, which indicate an oppoſite power in animals; whereby they are capable of reſiſting any external cold while alive, by generating within themſelves a degree of heat ſufficient to counteract it. Theſe experiments were not originally inſtituted in view of the diſcovery, which in the event reſulted from them, but for a very different purpoſe; which was no other than to ſatisfy myſelf, whether an animal could retain life after it was frozen, as had been confidently aſſerted both of fiſh and ſnakes. I mention this, to account for what might otherwiſe be attributed to negligence and inattention; namely, that little nicety was uſed in meaſuring the preciſe degrees of the cold applied in theſe experiments. Accuracy in this particular was not aimed at, being of no conſequence in the inquiry more immediately before me. The cold produced was firſt by means of ice and ſnow with *ſal ammoniac* or ſea-ſalt, and was about 10° of FAHRENHEIT's thermometer. Then ice, ſo cooled, was mixed with ſpirit of nitre; but what degree of cold was thus produced I did not examine. This cold mixture was made in a tub ſurrounded with woollen cloths, and covered with the ſame, to prevent the effects of the heat of the atmoſphere upon the mixture itſelf, and to preſerve as much as poſſible a cold atmoſphere within the veſſel. The animal juices, the blood for example, freeze at 25°; ſo that a piece of dead fleſh could be frozen in ſuch an atmoſphere.

EXPERIMENTS.

I. The firſt experiment was made on two carp. They were put into a glaſs veſſel with common river water, and the veſſel put into the freezing mixture; the water did not freeze faſt enough; and therefore, to make it freeze ſooner, we put in as much cooled ſnow as to make the whole thick. The ſnow round the carp melted; we put in more freſh ſnow, which melted alſo; and this was repeated ſeveral times, till we grew tired, and at laſt left them covered up in the yard, to freeze by the joint operation of the ſurrounding mixture and the natural cold of the atmoſphere. They were frozen at laſt, after having exhauſted the whole powers of life in the production of heat. That this was really the caſe, could not be known, till I had compleated that part of the experiment,

riment, for which the whole was begun; *viz.* the thawing of the animals. This was done very gradually; but the animals did not with flexibility recover life. While in this cold, they ſhewed ſigns of great uneaſineſs by their violent motions. In ſome of theſe experiments, where air was made the conductor of the cold and heat, that the heat might be more readily carried off from the animal, a leaden veſſel was uſed. It was ſmall for the ſame reaſon; and as it was neceſſary, for the animal's reſpiration, that the mouth of the veſſel ſhould communicate with the open air, it was made pretty deep, that the cold of the atmoſphere round the animal might not be diminiſhed faſt by the warmth of the open air, which would have ſpoiled it as a conductor.

II. The ſecond experiment was upon a dormouſe. The veſſel was ſunk in the cold mixture almoſt to its edge. The atmoſphere round the animal ſoon cooled; its breath froze as it came from the mouth; an hoar-froſt gathered on its whiſkers, and on all the inſide of the veſſel; and the external ends of the hair became covered with the ſame. While this was going on, the animal ſhewed ſigns of great uneaſineſs: ſometimes it would coil itſelf into a round form, to preſerve its extremities, and confine its heat; but finding that ineffectual, it then endeavoured to make its eſcape(a); its motions became leſs violent by the ſinking of the vital powers; and its feet were frozen; but we were not able to keep up the cold a ſufficient time to freeze the whole animal, its hair being ſuch a bad conductor of heat, that the conſumption was not more, than the animal powers were capable of ſupporting(b).

(a) This ſhews, that cold, carried to a great degree, rather rouſes the animal into action than depreſſes it; but it would appear, from many circumſtances and obſervations, that a certain degree of cold produces inactivity both in the living and ſenſative principle, which will be farther illuſtrated hereafter.

III. The third experiment was made upon another dormouſe. From the failure of the laſt experiment, I took care that the hair ſhould not a ſecond time be an obſtruction to the ſucceſs of our experiment. I therefore firſt wetted it all over, that the heat of the animal might be more inſtantaneouſly carried off; and then it was put into the leaden veſſel. The whole was put into the cold mixture as before. The animal ſoon gave ſigns of its feeling the cold, by repeated attempts to make its eſcape. The breath, and the evaporating water from its body were ſoon frozen, and appeared like a hoar-froſt on the ſides of the veſſel, and on its whiſkers; but while the vigour of life laſted, it defied the approach of the cold. However, from the hair being wet, and thereby rendered a good conductor of heat, there was a much greater conſumption of it than in the former experiment. This haſtened on a diminution of the power of producing it. The animal died, and ſoon became ſtiff; upon thawing it, we found it was dead.

(b) Theſe experiments were made in preſence of Dr. GEORGE FORDYCE and Dr. ERWIN, teacher of Chemiſtry at Glaſgow; the latter of whom came in accidentally in the middle of our operations.

IV. The fourth experiment was upon a toad. It was put into water juſt deep enough not to cover its mouth, and the whole was put into the cold mixture, now between 10° and 15°. It allowed the water to freeze cloſe to it, which as it were cloſed it in; but the animal did not die, and therefore was not frozen: however, it hardly ever recovered the uſe of its limbs.

V. The fifth experiment was with a ſnail, which froze very ſoon, in a cold between 10° and 13°; but this experiment was made in the winter, when the living powers of thoſe animals are very weak: it might have reſiſted the cold more ſtrongly in the ſummer.

To aſcertain whether vegetables could be frozen, and afterwards retain all their properties when thawed, or had the ſame power of generating heat with animals, I made ſeveral experiments. Vegetable juices when ſqueezed out of a green plant, ſuch as cabbage and ſpinnage, froze in a cold about 29°; and between 29° and 30° thawed again, which is about 4° above the point at which the animal juices freeze and thaw.

I. I took a young growing bean, about three inches long in the ſtalk, and put it into the leaden veſſel with common water, and then immerſed the whole into the cold mixture. The water very ſoon froze all round it; however, the bean itſelf took up a longer time in freezing than the ſame quantity of water would have done; yet it did freeze, and was afterwards thawed, and planted in the ground, but it ſoon withered. The ſame experiment was made upon the bulbous roots of tulips, and with the ſame ſucceſs.

II. A young Scotch fir, which had two compleat ſhoots and a third growing, and which conſequently was in its third year, was put into the cold mixture which was between 15° and 17°. The laſt ſhoot froze with great difficulty, which appeared to be owing in ſome meaſure to the repulſion between the plant and the water. When thawed, the young ſhoot was found flaccid. It was planted; the firſt and ſecond ſhoot we found retained life, while the third, or growing ſhoot, withered.

III. A young ſhoot of growing oats with three leaves, had one of the leaves put into the cold mixture at 22°, and it ſoon was frozen. The roots were next put in, but did not freeze; and when put into the ground, the whole grew, excepting the leaf which had been frozen. The ſame experiment was made upon the leaves and roots of a young bean, and attended with the ſame ſucceſs.

IV. A leaf taken from a growing bean was put into the cold mixture, and frozen, and afterwards thawed, which ſerved as a ſtandard. Another freſh leaf was taken and bent in the middle upon itſelf; a ſmall ſhallow leaden veſſel was put upon the top of the cold mixture, and the two leaves put upon its bottom; but one-half of each leaf was not allowed to touch the veſſel by the bend; the cold mixture was between 17° and 15°, and the atmoſphere at 22°. The ſurfaces of the two leaves which were in contact with the lead were ſoon frozen in both; but thoſe ſurfaces which roſe at right angles, and were therefore only in contact with the cold atmoſphere, did not freeze in equal times; the one that had gone through this proceſs before, froze much ſooner than the freſh one. The above experiment was repeated when the cold mixture was at 25°, 24°, and the atmoſphere nearly the ſame, and with the ſame ſucceſs; only the leaves were longer in freezing, eſpecially the freſh leaf.

V. The vegetable juices above mentioned being frozen in the leaden veſſel, the cold mixture at 28°, and the atmoſphere the ſame, a growing fir-ſhoot was laid upon the ſurface, alſo a bean-leaf; and upon remaining there ſome minutes, they were found to have thawed the ſurface on which they lay. This I thought might ariſe from the greater warmth of theſe ſubſtances at the time of application; but by moving the fir-ſhoot to another part, we had the ſame effect produced.

VI. A freſh leaf of a bean was exactly weighed; it was then put into the cold atmoſphere and frozen. In this ſtate it was put back into the ſame ſcale, and allowed to thaw. No alteration in the weight was produced.

It appears from the above experiments, that an animal muſt be deprived of life before it can be frozen. Secondly, that there is an exertion, or an expence of animal powers, in doing this, in proportion to the neceſſity; and that the whole animal life may be exhauſted in this way. Thirdly, that this power is in proportion to the perfection of the animal, the natural heat proper to each ſpecies, and to each age. It may alſo perhaps depend, in ſome degree, on other circumſtances not hitherto obſerved: for from experiment II. and III. upon dormice, I found that in theſe animals, which are of a conſtitution to retain nearly the ſame heat in all temperatures of the air, it required the greateſt cold I could produce to overcome this power; while in experiment IV. and V. this power in the toad and ſnail, whoſe natural heat is not always the ſame, but is altered very materially according to the external heat or cold, was exhauſted in a degree of cold not exceeding 10° or 15°: and the ſnail being the moſt imperfect of the two, its powers of generating heat were by much the weakeſt.

That the imperfect animals will allow of a conſiderable variation in their temperature of heat and cold, is proved by the following experiments. The thermometer being at 45°, having introduced the ball by the mouth into the ſtomach of a frog, which had been expoſed to the ſame cold, it roſe to 49°. I then put the frog into an atmoſphere made warm by heated water, and allowed it to ſtay there twenty minutes; when, upon introducing the thermometer into the ſtomach, it raiſed the quickſilver to 64°. But to what degree the more imperfect animals are capable of being rendered hotter and colder, at one time then another, I have not been able to determine. The torpidity of theſe animals in our winter is probably owing to the great change wrought in their by the external heat and cold. The cold in their bodies is carried to ſuch a degree, as in great meaſure to put a ſtop, while it laſts, to the vital functions. In warmer climates no ſuch effect is produced. In this reſpect they reſemble vegetables.

From the foregoing experiments it appears; firſt, that plants when in a ſtate of actual vegetation, or even in ſuch a ſtate as to be capable of vegetating under certain circumſtances, muſt be deprived of their principle of vegetation before they can be frozen. Secondly, Vegetables have a power within themſelves of producing or generating heat; but not always in proportion to the diminution of heat by application of cold, ſo as to retain at all times an uniform degree of heat: for the internal temperature of vegetables is ſuſceptible of variations to a much greater extent indeed than that of the more imperfect animals; but ſtill within certain limits. Beyond theſe limits the principle of vegetable, as of animal life, reſiſts any further change. Thirdly, the heat of vegetables varies, according to the temperature of the medium in which they are, which we diſcover by varying that temperature, and obſerving the heat of the vegetable. Fourthly, the expence of the vegetating powers in this caſe is proportioned to the neceſſity, and the whole vegetable powers may be exhauſted in this way. Fifthly, this power is moſt probably in proportion to the perfection of the plant, the natural heat proper to each ſpecies, and the age of each individual. It may alſo perhaps depend, in ſome degree, on other circumſtances not hitherto obſerved; for in experiment II. the old ſhoot did not loſe its powers, while that which was young or growing did; and in experiment III. and IV. we found, that the young growing ſhoot of the fir was with great difficulty frozen at 10°, while a bean-leaf was eaſily frozen at 22°; and in experiment V. the young ſhoot of the fir thawed the ice at 28°, much faſter than the leaf of the bean. Sixthly, it is probably, by means of this principle, that vegetables are adapted to different climates. Seventhly, that ſuſpenſion of the functions of vegetable life, which takes place during the winter ſeaſon, is probably owing to their being ſuſceptible of ſuch a great variation of internal temperature. Eighthly, the roots of vegetables are capable of reſiſting cold more than the ſtem or leaf; therefore, though the ſtem be killed by cold, the root may be preſerved, as daily experience evinces. The texture of vegetables alters very much by the loſs of life, eſpecially thoſe which are watry and young; from being brittle and criſp they become tough and flexible. The leaf of a bean when in full health is thick and maſſy, repels water as if greaſy, and will often break before it is conſiderably bent; but if it is killed ſlowly by cold, it will loſe all theſe properties, becoming then pliable and flaccid; deprived of its power of repelling water, it is eaſily made wet, and appears like boiled greens. If killed quickly, by being frozen immediately, it will remain in the ſame ſtate as when alive; but upon thawing, will immediately looſe all its former texture. This is ſo remarkable, that it would induce one to believe, that it loſt conſiderably of its ſubſtance; but from experiment VI. it is evident that it does not. The ſame thing happens to a plant when killed by electricity (c). If a growing juicy plant receives a ſtroke of electricity ſufficient to kill it, its leaves droop, and the whole becomes flexible.

So far animal and vegetable life appear to be the ſame; yet an animal and a vegetable differ in one very material circumſtance, which it may be proper to take particular notice of in this place, as it ſhews itſelf with remarkable evidence in theſe experiments. An animal is equally old in all its parts, excepting where new parts are formed in conſequence of diſeaſes; and we find, that theſe new or young parts in animals, like the young ſhoots of vegetables, are not able to ſupport life equally with the old; but every plant has in it a ſeries of ages. According to its years, it has parts of all the ſucceſſive ages from its firſt formation; each part having powers equal to its age, and each part, in this reſpect, being ſimilar to animals of ſo many different ages. Youth in all caſes is a ſtate of imperfection; for we find that few animals that come into the world in winter live, unleſs they are particularly taken care of; and we may obſerve the ſame of vegetables. I found that a young plant was more eaſily killed than an old one; as alſo the youngeſt part of the ſame plant.

(c) To kill a whole plant by electricity, it is neceſſary to apply the conductor, or give a ſhock to every projecting part; for any part that is out of the line of direction will ſtill retain life.

This power of generating heat ſeems to be peculiar to animals and vegetables while alive. It is in both a power only of oppoſition and reſiſtance; for it is not found to to exert itſelf ſpontaneouſly and unprovoked; but muſt always be excited by the energy of ſome external frigorific agent. In animals it does not depend on the motion of the blood, as ſome have ſuppoſed, becauſe it belongs to animals who have no circulation; beſides, the noſe of a dog, which is nearly always of the ſame heat in all temperatures of the air, is well ſupplied with blood: nor can it be ſaid to depend upon the nervous ſyſtem, for it is found in animals that have neither brain or nerves. It is then moſt probable, that it depends on ſome other principle peculiar to both, and which is one of the properties of life; which can, and does, act independently of circulation, ſenſation, and volition; *viz.* that power which preſerves and regulates the internal machine, and which appears to be common to animals and vegetables. This principle is in the moſt perfect ſtate when the body is in health, and in many deviations from that ſtate, we find that its action is extremely uncertain and irregular; ſometimes riſing higher than the ſtandard, and at other times falling much below it. Inſtances of this we have in different diſeaſes, and even in the ſame diſeaſe, in very ſhort intervals of time. A very remarkable one fell under my own obſervation, in a gentleman who was taken with an apoplectic fit; while he lay inſenſible in bed, and covered with blankets, I found that his whole body would, in an inſtant, become extremely cold in every part; continue ſo for ſome time; and, in as ſhort a time, he would become extremely hot. While this was going on for ſeveral hours alternately, there was no ſenſible alteration in his pulſe.

XLIV. *A Compariſon of the Heat of* London *and* Edinburgh. *By* John Roebuck, *M. D. F. R. S. in a Letter to* William Heberden, *M. D. F. R. S.*

SIR,

Redde, June 29, 1775. I DELIVERED to you ſome time ago, a regiſter of the thermometer at Hawkhill for ten years; but as theſe obſervations were made at eight o'clock in the morning and four in the afternoon, and yours at eight o'clock in the morning and two in the afternoon, the correſponding years of the morning's obſervations only admit of a compariſon. It appears by your regiſter, that the mean heat at London for nine years, from the end of 1763 to the end of 1772, at eight o'clock in the morning, was 47°.4; and the mean heat at Hawkhill, during the ſame period of time, was 46°. The difference of which is only 1°.4. A difference much leſs than might be expected from the difference of latitude, and not ſufficient to account why nonpareils, golden rennets, peaches, nectarines, and many kinds of grapes, generally come to maturity near London, and ſcarce ever near Edinburgh, without the aid of artificial heat. Before I proceeded further to perplex myſelf with this difficulty, I procured from Hawkhill and from yourſelf the regiſter of the thermometer for three years, at the ſame periods of time; copies of which I here incloſe you. And by theſe it appears, that the mean heat of London of theſe three years exceeded that of Edinburgh by 4°.5. And the mean heat of the three hotteſt months in London exceeded the mean of the ſame three at Edinburgh by 5°.8. And the mean heat of theſe three ſummer months, at two o'clock in the afternoon, in London exceeded the mean heat of the ſame months, at the ſame hour, in Edinburgh by 7°.3; which ſufficiently accounts why ſome fruit may come to maturity in one country and not in the other: and alſo why corn and graſs, which vegetate with a more temperate heat, but require a longer continuance of it, may arrive at maturity in both countries. The reaſon why the mean heat of London exceeds that of Edinburgh may ariſe principally from the difference of latitude. But the reaſon why the exceſs is greater in proportion in the three hotteſt months of the year, at the hotteſt time of the day, than in the winter months, ariſes from Edinburgh's being ſituated nearer to the ſea than London. We might ſpeak with more preciſion on this ſubject, if we had a regiſter of the thermometer at Moſcow, which is nearly of the ſame latitude as Edinburgh; though it is well known, that the heat of ſummer is much more intenſe, and the cold of winter much more ſevere, at Moſcow than at Edinburgh. The mean heat of ſprings near Edinburgh ſeems to be 47°; and at London 51°. It is probable, that the mean heat of good ſprings in any country is very nearly the mean heat of the country (a). A faithful account of the heat of ſprings in different latitudes, and of water taken from the ſame depth of the ſea in different latitudes is yet wanted.

I am, &c.

(a) We ſhall have an eaſy method of finding the mean heat of any place, if it be always nearly equal to that of its ſprings. This matter might be aſcertained by a proper number of obſervations; and it is therefore very deſireable, to have an account taken of the heat of the ſprings, wherever a regiſter is kept of the heat of the air. W. HEBERDEN.

Mean Heat in PALL MALL, LONDON.

	1772.		1773.		1774.		Mean heat of Three Years.	
	8 A.M.	2 P.M.	8 A.M.	2 P.M.	8 A.M.	2 P.M.	8 A.M.	2 P.M.
January	36	38	42	44	34	39	37.3	40.3
February	38	42	36	41	38	44	37.3	42.3
March	41	47	40	51	41	52	40.7	50
April	44	51	45	55	47	55	45.3	53.7
May	49	60	50	60	51	60	50	60
June	64	73	58	67	59	67	60.3	69
July	61	72	60	68	61	69	60.7	69.7
Auguſt	60	70	62	72	62	70	61.3	70.7
September	56	65	56	63	55	63	55.7	63.7
October	56	61	51	59	48	58	51.7	59.3
November	45	55	40	47	40	44	41.7	48.7
December	41	44	41	45	39	43	40.3	44
Mean	49.2	56.5	48.4	56	47.9	55.3	48.5	56
Mean heat of three years morning and afternoon was 52.2.								

Mean heat at HAWKHILL, ſituated about one mile North of Edinburgh, and 103 feet above the level of the ſea.

	1772.		1773.		1774.		Mean Heat of Three Years.	
	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.	8 A. M.	2 P. M.
January	31.5	34.3	38.5	40.3	29.1	33	33.3	35.8
February	30.9	36.5	35.1	40.7	36.2	40.4	34	39.2
March	37	42.8	42.1	48.4	37.1	43.2	38.7	44.8
April	42.9	48.5	45.6	51.1	44.1	48.9	44.2	49.5
May	49.1	54.5	48.6	53.1	46.6	50.8	48.1	52.8
June	57.2	62.1	55.2	60.1	51.1	59.7	54.5	60.6
July	58.7	64.6	57.7	61.9	57.4	63.3	57.9	63.3
Auguſt	57.4	63.9	58.3	64.8	57.2	62.5	57.6	63.7
September	51.5	58.1	51.3	55.8	51.7	57.8	51.5	57.2
October	48.8	51.6	46	50.7	48.3	52.8	47.7	51.7
November	41.7	44.6	38.2	42.3	38	42	39.3	42.9
December	39.7	41.6	36.4	38.5	37.3	40	37.8	40
Mean heat,	45.5	50.3	46.1	50.6	44.5	49.5	45.4	50.1

Mean heat of three years morning and afternoon was 47.7.

XLV. *Experiments in an heated Room.* *By* Matthew Dobſon, *M. D.* *In a Letter to* John Fothergill, *M. D. F. R. S.*

DEAR SIR, Liverpool, April 25, 1775.

Redde, June 22, 1775.

I PERUSED with particular pleaſure, your ſhort account of the curious experiment made by Mr. BANKS and Dr. SOLANDER. The ſame, and ſome additional experiments, have been made here; the reſult of which I ſhould ſooner have tranſmitted to you, had I not been prevented by the conſtant engagements of my profeſſion.

EXPERIMENTS.

I. The ſweating-room of our Public Hoſpital at LIVERPOOL, which is nearly a cube of nine feet, lighted from the top, was heated till the quickſilver ſtood at 224° on FAHRENHEIT's ſcale, nor would the tube of the thermometer indeed admit the heat to be raiſed higher. The thermometer was ſuſpended by a ſtring fixed to the wooden frame of the ſky-light, and hung down about the centre of the room. Myſelf and ſeveral others were at this time incloſed in the ſtove, without experiencing any oppreſſive or painful ſenſation of heat, proportioned to the degree pointed out by the thermometer. Every metallic about us ſoon became very hot.

II. My

II. My friend Mr. PARK, an ingenious ſurgeon of this place, went into the ſtove heated to 202°. After ten minutes, I found the pulſe quickened to 120°. And to determine the increaſe of the animal heat, another thermometer was handed to him, in which the quickſilver already ſtood at 98°; but it roſe only to 99½, whether the bulb of the thermometer was incloſed in the palms of the hands, or received into the mouth(a). The natural ſtate of this gentleman's pulſe is about 65.

III. Another gentleman went through the ſame experiment in the ſame circumſtances, and with the ſame effects.

IV. One of the porters to the Hoſpital, a healthy young man, and the pulſe 75, was incloſed in the ſtove when the quickſilver ſtood at 210°; and he remained there, with little inconvenience, for twenty minutes. The pulſe, now 164, and the animal heat, determined by another thermometer as in the former experiments, was 101½.

V. A young gentleman of a delicate and irritable habit, whoſe natural pulſe is about 80, remained in the ſtove ten minutes when heated to 224°. The pulſe roſe to 145, and the animal heat to 102°. This gentleman, who had been frequently in the ſtove during the courſe of the day, found himſelf feeble, and diſpoſed to break out into ſweats for 24 hours after the experiment.

(a) The ſcale of the thermometer, which was ſuſpended by the ſtring about the middle of the room, was of metal; this was the only one I could then procure, on which the degrees ran ſo high as to give any ſcope to the experiment. The ſcale of the other thermometer, which was employed for aſcertaining the variations in the animal heat, was of ivory.

VI. Two ſmall tin veſſels, containing each the white of an egg, were put into the ſtove heated to 224°. One of them was placed on a wooden ſeat near the wall, and the other ſuſpended by a ſtring about the middle of the ſtove. After ten minutes, they began to coagulate; but the coagulation was ſenſibly quicker and firmer in that which was ſuſpended, than in that which was placed on the wooden ſeat. The progreſs of the coagulation was as follows: it was firſt formed on the ſides, and gradually extended itſelf; the whole of the bottom was next coagulated; and laſt of all the middle part of the top.

VII. Part of the ſhell of an egg was peeled away, leaving only the film which ſurrounds the white; and part of the white being drawn out, the film ſunk ſo as to form a little cup. This cup was filled with ſome of the *albumen ovi*, which was conſequently detached as much as poſſible from every thing but the contact of the air and of the film which formed the cup. The lower part of the egg ſtood upon ſome light tow in a common gallipot, and was placed on the wooden ſeat in the ſtove. The quickſilver in the thermometer ſtill continued at 224°. After remaining in the ſtove for an hour, the lower part of the egg which was covered with the ſhell, was firmly coagulated; but that which was in the little cup was fluid and tranſparent. At the end of another hour it was ſtill fluid, except on the edges where it was thinneſt; and here it was ſtill tranſparent; a ſufficient proof that it was dried, not coagulated.

VIII. A piece of bees wax, placed in the ſame ſituation with the *albumen ovi* of the preceding experiment, and expoſed to the ſame degree of heat in the ſtove, began to melt in five minutes: another piece ſuſpended by a ſtring, and a third piece put into the tin veſſel and ſuſpended, began likewiſe to liquify in five minutes.

OBSERVATIONS.

That heated air ſhould have ſuch a ſpeedy and powerful effect in quickening the pulſe, while the animal heat is little altered from its natural ſtandard; that the human body ſhould ſo eaſily bear to be ſurrounded with air heated to 224°; that the *albumen ovi*, which begins to coagulate in water at 150°, ſhould remain fluid in 224°; and that the ſame *albumen ovi*, ſtill placed in air heated to 224°, ſhould coagulate if in contact either with tin or its own ſhell, are facts as ſingular as they are difficult of explanation. From the different effects of heated air on the pulſe and the heat of the body, do we not diſcover the fallacy of that theory of animal heat which has been adopted by BOERHAAVE and other celebrated phyſiologiſts? They ſuppoſe that animal heat is produced by the attrition of the *globules* of the circulating fluids againſt the ſides of the containing veſſels; but in ſeveral of the preceding experiments, the circulation was amazingly quickened with little increaſe of the animal heat. But whence is it that the human body can bear without immediate injury, to be ſurrounded with air heated to 224°? And whence is it, that the *albumen ovi* does not coagulate in this degree of heat? Is it that fire as it paſſes into ſome bodies becomes latent, agreeable to a doctrine which has for ſome time been taught at Edinburgh by Profeſſor BLACK? Or does fire become fixed and *quieſcent*, according to a ſimilar ſyſtem adopted by Dr. FRANKLIN(b)? Air we know exiſts either in a fixed or elaſtic ſtate; and fire may in like manner exiſt in bodies, either in a latent, fixed, and *quieſcent*; or in a ſenſible, fluid, and active ſtate. Agreeable to this idea, the bees wax receives the fire in an active ſtate, and diſſolves; while the human body and the *albumen ovi*, receiving the fire in a latent ſtate, are little altered in their temperature. Let each of theſe, however, be put in contact with a different body, tin for inſtance; and though the heat of the air continues the ſame, yet the fire no longer enters in a latent ſtate, but with all its ſenſible and active powers; for the *albumen ovi* ſuſpended in a tin veſſel ſoon coagulates; and the human body, covered with the ſame metal, would quickly experience an intolerable and deſtructive degree of heat. Or are the above phænomena more ſatisfactorily explained, by conſidering different bodies as poſſeſſing different conducting powers; ſome being ſtrong, others weak conductors of fire? All thoſe bodies then which are weak conductors of fire from air, may be placed in air, without receiving the heat of this medium. Hence the *albumen ovi* remains fluid in air heated to 224°. Hence likewiſe the frog, the lizard, the camelion, &c. retain their natural temperature, and feel cold to the touch, though perpetually ſurrounded with air hotter than their own bodies. Hence alſo, the human body keeps nearly its own temperature, in a ſtove heated to 224°: or may even paſs without injury into air heated to a much greater degree, according to the obſervations of DU HAMEL and TILLETT, publiſhed in the Memoirs of the Academy of Sciences(c). On the other hand, all thoſe bodies which are powerful conductors of fire from air, are influenced in proportion when ſurrounded with this medium. The bees wax melted from the mere contact of the air in experiment VIII; and in experiment VI, the *albumen ovi* was coagulated on the intervention of another body, which is a ſtrong conductor of fire from air. But whether this method of reaſoning on the natural cauſe of theſe effects be juſt or not, the final cauſe is obvious, and is to be reſolved into the wiſe and benevolent appointment of the Almighty. Man is happily ſo framed, as to poſſeſs a power of keeping nearly the ſame tenor of heat, in all the variations of the temperature of the air in ſummer and in winter, in hot and cold climates; and conſequently changes his ſituation on the ſurface of the globe, with much leſs inconvenience or injury, than he could otherwiſe have done. The ſame power likewiſe happily adapts different animals to their reſpective deſtinations. The lizard and the camelion remain cool under the Equator, while the whale and porpoiſe retain a degree of heat above that of the human body, though ſurrounded with the waters of the coldeſt

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(b) Exper. and Obſerv. p. 346. and 412.

(c) Memoires pour 1761.

Northern ſeas, and amidſt mountains of ice in the neighbourhood of the Pole.

Should you think theſe experiments and obſervations on heated air of ſufficient importance to be communicated to the Royal Society, they are at your diſpoſal.

I have the pleaſure to find, that Dr. PRIESTLEY is proſecuting his very ingenious inquiries on air. In a letter I lately received from him, he informs me, that he has diſcovered a ſpecies of air, which will preſerve animal life ſix times longer than atmoſpheric air.

I remain, with great eſteem, &c.

XLVI. *Calculations in Spherical Trigonometry abridged. By* Iſrael Lyons. *In a Letter to Sir* John Pringle, *Bart. P. R. S.*

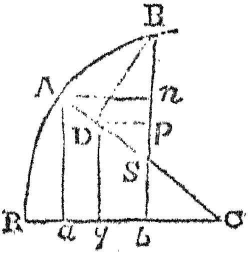
TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

Redde, July 6, 1775.

SINCE aſtronomical obſervations have been made with much greater preciſion than formerly, it became requiſite that the calculations correſponding to them ſhould likewiſe be made to much greater degrees of exactneſs. The ancient aſtronomers deſired only to make their obſervations and computations agree within a part of a degree; ſucceeding ones were ſatisfied when they correſponded within a minute; but no leſs exactneſs than ſeconds will content the moderns. The rules in ſpherical trigonometry being reduced to operations by logarithms, it is neceſſary to uſe ſuch a number of figures in the tables as will produce the required preciſion; this is very different in the various parts of the quadrant, inſomuch that if the arc is only one degree, four places of decimals in the logarithm of a ſine are ſufficient to determine the arc to which it belongs within a ſecond: whereas if the arc is 89°, there is a neceſſity of uſing eight figures for the ſame purpoſe: thus, the logarithm ſine of 89° 0′ 0″ is 9.9999338, the ſame ſeven figures as for the logarithm ſine of 89° 0′ 1″. From this conſideration it follows, that the analogies commonly laid down and uſed for the ſolutions of ſpherical triangles are not in all caſes equally convenient, and I might ſay, equally accurate; and that it would be more eaſy and exact in calculations to find what was required, by means of ſines of arcs, which, being ſmall, require the uſe of only a few places of figures. Now the caſes which often occur in aſtronomy, where ſpherical trigonometry can only be of uſe, are generally of ſuch a nature that we know nearly, or at leaſt within a few degrees, what the required ſide or angle is, there is nothing therefore wanted but to find how much this quantity, or firſt approximation, differs from the true value of the ſide or angle. Thus in calculating the right aſcenſion of any point of the ecliptic, whoſe longitude and declination are known, inſtead of finding the right aſcenſion immediately, it will be more convenient to ſeek for the difference between the longitude and right aſcenſion, which as it never exceeds $2\frac{1}{2}°$, four or five places of figures will always be ſufficient to determine it within a ſecond. And in other ſimilar caſes, rules might be made agreeable to the exigency of each particular caſe, which would be better than the application of the general method of ſolution. Some examples of which ſhall be ſhewn in the following paper: the deſign of which is to point out a method of ſolving ſeveral of the moſt uſeful queſtions in ſpherical trigonometry in a manner ſomewhat ſimilar to that uſed in approximating to the roots of algebraic equations. This method is founded on the following

LEMMA.



If the radius is ſuppoſed equal to unity, the ſine of the ſum of two arcs, α and β, is equal to ſin. α + coſ. $\alpha \times$ ſin. β − ſin. $\alpha \times$ verſ. ſin. β. And its coſine = coſ. α − ſin. $\alpha \times$ ſin. β − coſ. $\alpha \times$ verſ. ſin. β.

DEMONSTRATION.

Let the arc α be RA, and the arc β be AB, their ſines Aa, BD, reſpectively; then Bb being drawn perpendicular to the radius CR will be the ſine of $\alpha+\beta$. Draw Dp and An parallel to CR. Then, by ſimilar triangles, CA : Ca :: BD : Bp, and CA : Aa :: AD : np. Therefore, $Bb(=Aa+Bp-pn)=Aa+\frac{Ca\times BD}{CA}-\frac{Aa\times AD}{CA}$; that is, ſine $\overline{\alpha+\beta}$ = ſin. α + coſ. $\alpha \times$ ſin. β − ſin. $\alpha \times$ verſ. ſin. β.

In the ſame manner, drawing Dq parallel to Aa we may prove $Cb(=Ca-bq-aq)=CA-\frac{Aa\times BD}{CA}-\frac{Ca\times AD}{CA}$, or coſ. $\overline{\alpha+\beta}$ = coſ. α − ſin. $\alpha \times$ ſin. β − coſ. $\alpha \times$ verſ. β.

In what follows, for brevity ſake, the arc is expreſſed by a Greek letter; its ſine by the capital character; and the coſine by the ſmall italic character of the ſame letter. In this notation, the two theorems will ſtand thus, ſin. $\overline{\alpha+\beta}=\mathrm{A}+a\mathrm{B}-\mathrm{A}\times$ vſ. β, and coſ. $\overline{\alpha+\beta}=a-\mathrm{AB}-a\times$ vſ. β.

COROLLARY I.

Since the tangent is equal to the ſine divided by the coſine, we ſhall have

$$\text{Tang. }\overline{\alpha+\beta}=\frac{\mathrm{A}+a\mathrm{B}-\mathrm{A}\times\text{vſ. }\beta}{a-\mathrm{AB}-a\times\text{vſ. }\beta}=\frac{\mathrm{A}}{a}+\frac{\mathrm{B}}{a^2}+\frac{\mathrm{A}}{a^3}\times\text{ vſ. }\beta\text{ nearly.}$$

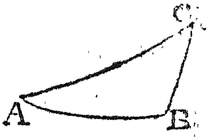
COROLLARY II.

If we change the ſign of β, we ſhall have ſin. $\alpha-\beta=\mathrm{A}-a\mathrm{B}-\mathrm{A}\times$ vſ. β. Coſ. $\alpha-\beta=a+\mathrm{AB}-a\times$ vſ. β. And tang. $\alpha-\beta=\frac{\mathrm{A}}{a}-\frac{\mathrm{B}}{a^2}+\frac{\mathrm{A}}{a^3}\times$ vſ. β.

By the help of theſe theorems, knowing nearly what any quantity in a ſpherical triangle is, we may find its correction, thus: if we have to find the coſine of an arc, which arc we know is nearly equal to α whoſe coſine is a. Suppoſe the arc to be $\alpha-\beta$, and its coſine $a+c$. Then $a+c=$ coſ. $\alpha-\beta=a+\mathrm{AB}-a\times$ vſ. β. Therefore, $\mathrm{B}=\frac{c}{\mathrm{A}}+\frac{a}{\mathrm{A}}\times$ verſ. β.

The firſt term $\frac{c}{A}$ will always give a near approximation to the value of ſin. β, and β being found the correction, $\frac{a}{A} \times$ vſ. β, or cot. $\alpha \times$ vſ. β, may be found and added to it. Among the tables requiſite to be uſed with the Nautical Almanac, is table IV. for parallax, p. 19. which ſhews the value at ſight of ſuch quantities as vſ. $\beta \times$ cot. α, the arc β being found in the firſt column of the table, and α at the top. This table I have calculated only to arcs under 63′; but it would be found uſeful to have a table ready computed for all arcs under 5°.

PROBLEM I.



If the two legs, AB *and* BC, *of the ſpherical triangle* ABC *right-angled at* B, *are given, to find the hypotenuſe* AC, *the leg* BC, *being ſmall in compariſon of* AC.

Let AB $=\alpha$, BC $=\beta$, and ſuppoſe AC $=\alpha+\zeta$, α being a near approximation to AC, and ζ the ſmall arc to be added to AB to make it equal to AC; then coſ. AC = coſ. AB × coſ. BC; that is, according to our notation, $a - \mathrm{AZ} - \overline{a \times \text{vſ.}\ \zeta} = ab$.

Whence $\mathrm{Z} = \frac{a-ab}{\mathrm{A}} - \overline{\frac{a}{\mathrm{A}} \times \text{vſ.}}\ \zeta = \text{cot.}\ \alpha \times \text{vſ.}\ \beta - \text{cot.}\ \alpha \times \text{vſ.}\ \zeta$.

EXAMPLE.

Let AB be 75° 0′ and BC 20° 0′, and the computation will be as follows:

Cotangent AB		9.4280
Verſed ſine BC		8.7804
ζ nearly	55′ 33″ ſine	8.2084
Correction	−7	from tab. IV. Nautical Almanac.

Therefore $\zeta =$ 55 26 and AC = 75° 55′ 26″.

By this problem, the diſtance of the Sun may be found from a planet whoſe latitude and difference of longitude are known.

PRO-

PROBLEM II.

Having the hypotenuſe AC *and one of the angles* A, *to find the baſe* AB.

Let AC$=\beta$, BAC$=\alpha$, and ſuppoſe AB$=\beta-\zeta$, then coſ. A$=$ cot. AC $\times$ tang. AB, or $a=\frac{b}{B}\times\frac{B}{b}-\frac{z}{b^2}+\frac{B\times \text{vſ.}\,\zeta}{b^3}=1-\frac{z}{Bb}+\frac{B\times\text{vſ.}\,\zeta}{b^2}$.

Whence $z=Bb\times\overline{1-a}+\frac{B}{b}\times\text{vſ.}\,\zeta=\frac{1}{2}\,\text{ſin.}\,2\beta\times\text{vſ.}\,\alpha+\text{tang.}\,\beta\times\text{vſ.}\,\zeta$.

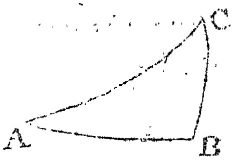
EXAMPLE.

Let A$=23°\ 28'\ 15''$, and AC$=10°\ 0'\ 0''$.

Sine 2 AC 20° 0′		9.5340
Verſed ſine A		8.9177
		8.4517
Log. 2.		0.3010
ζ nearly	48′ 39″ ſine	8.1507
Correction	+ 6	
ζ	48 45 and BC=159° 11′ 15″.	

By this problem, the right aſcenſion of any point of the ecliptic, whoſe obliquity and longitude are known, may be found.

PROBLEM III.



Suppoſing the ſame things known as in the laſt, to find the perpendicular BC, *when the hypotenuſe is nearly a quadrant.*

Let $\text{A}=\alpha$, $\text{AC}=\beta$, as before, and ſuppoſe $\text{BC}=a-\zeta$; then ſin. BC = ſin. AC × ſin. A, or $\text{A}-a z-\text{A}\times$ vſ. $\zeta=\text{AB}$, whence $z=\frac{\text{A}-\text{BA}}{a}-\frac{\text{A}}{a}\times$ vſ. $\zeta=$ tang. $\alpha\times$ co. ver. ſin. $\beta-$ t. $\alpha\times$ vſ. ζ.

EXAMPLE.

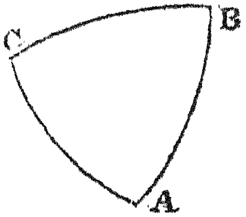
Let $\text{A}=23^\circ\ 28'\ 15''$, and $\text{AC}\times 80^\circ\ 0'$.

Tang. A		9.6377
Verſ. ſin. co. AC, 10°		8.1816
ζ nearly	22° 41 ſine	7.8193
Correction	− 1	
ζ	22 40 and BC=23° 5′ 35″.	

This problem will be of uſe to find the declination of the ecliptic, and the latitude of a planet near the limits.

Theſe three inſtances will ſuffice for an application of this method to right-angled ſpherical triangles; we ſhall now give two problems of oblique triangles.

PROBLEM IV.



Suppoſe ABC *to be a ſpherical triangle, in which are given the two ſides* AB, BC, *with the included angle* B, *to find the third ſide* AC.

SOLUTION I.

Let $\mathrm{ABC}=\beta$, $\mathrm{BC}=\alpha$, $\mathrm{AB}=\delta$. Put $\mathrm{AC}=\beta+\zeta$, β being an approximate value of AC, when the two legs are nearly quadrants. Now the coſine of AC being equal to $b\,\mathrm{DA}+da$(a) we ſhall have $b-\mathrm{BZ}-\overline{b\times\text{vſ.}\,\zeta}=b\,\mathrm{DA}+da$: and $\mathrm{Z}=\frac{b-b\,\mathrm{DA}-da}{\mathrm{B}}-\frac{b}{\mathrm{B}}\times\text{vſ.}\,\zeta$. But $1-\mathrm{DA}-da=\text{vſ.}\,\delta-\alpha$, which put $=w$. Then $\mathrm{Z}=\frac{bw}{\mathrm{B}}+\frac{b\,da-da}{\mathrm{B}}-\frac{b}{\mathrm{B}}\times\text{vſ.}\,\zeta=\text{cot.}\,\beta\times\text{vſ.}\,\overline{\delta-\alpha}-\text{coſ.}\,\delta\times\text{coſ.}\,\alpha\times\text{tang.}\,\tfrac{1}{2}\beta-\text{cot.}\,\beta\times\text{vſ.}\,\zeta$. Therefore ζ is the difference of two arcs whoſe ſines are $\text{cot.}\,\beta\times\text{vſ.}\,\delta-\alpha$, and $\text{coſ.}\,\delta\times\text{coſ.}\,\alpha\times\text{tang.}\,\tfrac{1}{2}\beta$, the difference of theſe two arcs being diminiſhed by the correction $\text{cot.}\,\beta\times\text{vſ.}\,\zeta$.

(a) It is a well known theor. that $\text{ſin.}\,\mathrm{BA}\times\text{ſin.}\,\mathrm{BC}:r^2=\text{vſ.}\,\mathrm{AC}-\text{vſ.}\,\overline{\mathrm{AB}-\mathrm{BC}}:\text{vſ.}\,\mathrm{B}$; that is, $\text{ſin.}\,\mathrm{BA}\times\text{ſin.}\,\mathrm{BC}:r^2=\text{coſ.}\,\overline{\mathrm{AB}-\mathrm{BC}}-\text{coſ.}\,\mathrm{AC}:r-\text{coſ.}\,\mathrm{B}$. Or, in the author's notation, putting $r=1$, $\mathrm{DA}:1=\text{coſ.}\,\overline{\delta-\alpha}-\text{coſ.}\,\mathrm{AC}:1-b$. Therefore $\mathrm{DA}-b\,\mathrm{DA}=\text{coſ.}\,\overline{\delta-\alpha}-\text{coſ.}\,\mathrm{AC}$. Or, $\text{coſ.}\,\mathrm{AC}=b\,\mathrm{DA}-\mathrm{DA}-\text{coſ.}\,\overline{\delta-\alpha}$. For $\text{coſ.}\,\overline{\delta-\alpha}$ ſubſtitute its value as expreſſed in the ſecond corollary of the lemma, and there ariſes the author's equation, $\text{coſ.}\,\mathrm{AC}=b\,\mathrm{DA}+da$.

S. HORSLEY.

EXAMPLE.

Suppoſe $\mathrm{B} = 51^\circ\ 12'\ 5''$
$\mathrm{AB} = 87\ \ 57\ \ 51$
$\mathrm{BC} = 87\ \ 20\ \ 34$

Cotangent B	9.9053	Tang. $\frac{1}{2}$ B 25° 36′	9.6804
Verſ. ſine AB—BC 0° 37′ 17″	5.7693	Coſine AB	8.5506
		Coſine BC	8.6661
1ſt arc 0′ 10″ ſine	5.6746	2d arc 2′ 43″ ſine	6.8971

The difference of theſe two arcs, 2′ 33″
Subtracted from the value of the angle B, 51 12 5

Leaves AC, 51 9 32
The correction cot. $\beta \times$ vſ. ζ in this example is 0.

This ſolution is very convenient to find the diſtance of two Zodiacal Stars, having their latitudes and difference of longitude.

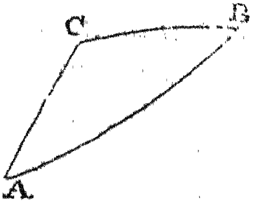
SOLUTION II.

Let τ be an arc whoſe coſine $t = b \times$ coſ. $\delta - \alpha = bda + b\mathrm{DA}$, and ſuppoſe $\mathrm{AC} = \tau - \zeta$, then $t + \mathrm{TZ} - t \times$ vſ. $\zeta = b\mathrm{DA} + da$ $= t - bda + da$. Whence $\mathrm{Z} = da \times \frac{1-b}{\mathrm{T}} + \frac{t}{\mathrm{T}} \times$ verſ. $\zeta =$ coſec. τ $\times$ coſin. $\alpha \times$ coſin. $\delta \times$ vſ. $\beta +$ cot. $\tau \times$ vſ. ζ.

This ſolution is uſeful to find the diſtance of the Moon from a ſtar at ſome diſtance from the ecliptic, in which caſe it coincides with the rule given by the Aſtronomer

Royal, Phil. Tranſ. 1764, vol. LIV. and which taking in the correction here given cot. $\tau \times$ vſ. ζ will always be exact to a ſecond. It is alſo of uſe to find the declination of a ſtar, whoſe longitude and latitude and obliquity of the ecliptic are known.

SOLUTION III.



Let the angle B be ſmall, and the two legs AB, BC, very unequal; then the ſide AC will be nearly AB−BC. Put this $=\varkappa$, and ſuppoſe AC $=\varkappa+\zeta$, then coſ. AC $= k - \mathrm{KZ} - k \times$ vſ. $\zeta = ad + \mathrm{AD} - \mathrm{KZ} - k \times$ vſ. $\zeta = b\,\mathrm{AD} + ad$, Whence $\mathrm{Z} = \frac{\mathrm{DA} - b\,\mathrm{AD}}{\mathrm{K}} - \frac{k}{\mathrm{K}} \times$ vſ. $\zeta =$ ſin. $\delta \times$ ſin. $\alpha \times$ vſ. $\beta \times$ coſec. $\delta - \alpha -$ cot. $k \times$ vſ. ζ.

EXAMPLE.

Let AB $= 94° 36' 58''$
BC $= 23\ 28\ 24$
B $= 24\ 54\ 24$
as in the example to ſol. 2.

AB−BC $= 71\ 8\ 34$	Coſecant		0.02396
Sine AB			9.99859
Sine BC			9.60023
Verſed of B			8.96851
ζ nearly $2° 14' 11''$ ſine			8.59129

The value of ζ being without the limits of tab. IV. in the tables requiſite to be uſed with the Nautical Almanac, the correction cot. $\kappa \times$ vſ. ζ muſt be computed thus:

Cot. κ	9.533
V. ſin. ζ	6.881

Cor. 0′ 53″. ſine 6.414, this ſubtracted from the firſt value of ζ, leaves $\zeta = 2° \ 13' \ 18''$, which added to $\delta - \alpha$, gives the ſide AC$=73° \ 21' \ 52''$. This ſolution will help to find the Sun's altitude near noon.

I have dwelt the longer on this problem becauſe it is one that is very commonly required in aſtronomical calculations, and the operation by the rules of ſpherical trigonometry in this as well as the next is rather troubleſome.

PROBLEM V.

Suppoſing the ſame things given, to find either of the angles, as for inſtance C *oppoſite the ſide* AB.

We have cot. C = cot. B × coſ. BC − ſin. BC × cot. AB × coſec. B $=\frac{ba}{\mathrm{B}}-\frac{\mathrm{A}d}{\mathrm{BD}}$. Let μ be an angle whoſe cot. $\frac{m}{\mathrm{M}}$ = cot. β × ſin. $\delta-\alpha$ × coſec. $\delta=\frac{ba\mathrm{D}-b\mathrm{A}d}{\mathrm{BD}}$, and ſuppoſe C $=\mu+\zeta$, then cot C $=\frac{m}{\mathrm{M}}-\frac{\mathrm{z}}{\mathrm{M}^2}+\frac{m.\,\text{vſ.}\,\zeta}{\mathrm{M}^3}=\frac{ba\mathrm{D}-\mathrm{A}d}{\mathrm{BD}}$. Whence $\mathrm{z}=\mathrm{M}\times\frac{\mathrm{A}d-b\mathrm{AD}}{\mathrm{BD}}+\frac{m}{\mathrm{M}}\times\text{vſ.}\,\zeta=\overline{\text{ſin.}\,\mu}^{2}\times\text{ſin.}\,\alpha\times\text{cot.}\,\delta\times\text{tang.}\,\tfrac{1}{2}\beta+\text{cot.}\,\mu\times\text{vſ.}\,\zeta$.

EXAMPLE.

Let	AB=94° 36′ 58		
	BC=23 28 54		
	B=24 54 24	Cotang.	0.3331770
Diff. AB and BC=	71 8 34	Sine	9.9760412
		Coſecant AB	0.0014080
	μ= 26 3 44	Cot.	10.3166262

$\overline{\text{Sin.}\ \mu}^{2}$	9.286
Sin. BB	9.600
Cot. AB	8.909
Tang. ½B	9.344

ζ=4′ 44″ ſine 7.139, this ſubtracted from μ leaves the angle C=25° 59′ 0″

This problem will be of uſe to find the right aſcenſion of a ſtar whoſe longitude and latitude, and obliquity of the ecliptic are known, or to find the Sun's azimuth at any hour in a given latitude.

I have added no cautions when theſe approximations and corrections change their ſigns, becauſe any mathematician will diſcover them at ſight.

I have the honour to be, &c.

XLVII. *Further Experiments and Obſervations in an heated Room.* *By* Charles Blagden, *M. D. F. R. S.*

Redde, July 6, 1775.

ON the third of April, nearly the ſame party as before (a), together with Lord SEAFORTH, Sir GEORGE HOME, Mr. DUNDAS, and Dr. NOOTH, went to the heated room in which the experiments of the 23d of January were made. Dr. FORDYCE had ordered the fire to be lighted the preceding day, and kept up all night; ſo that every thing contained in the room, and the walls themſelves, being already well warmed, we were able to puſh the heat to a much higher degree than before. In the courſe of the day ſeveral different ſets of experiments were going on together; but to avoid confuſion, it will be neceſſary to relate each ſeries by itſelf, without regard to the order of time; beginning with that ſeries which ſerves as a continuation of our former experiments.

Soon after our arrival, a thermometer in the room roſe above the boiling point; this heat we all bore perfectly well, and without any ſenſible alteration in the temperature of our bodies. Many repeated trials, in ſucceſſively higher degrees of heat, gave ſtill more remarkable proofs of our reſiſting power. The laſt of theſe experiments was made about eight o'clock in the evening, when the heat was at the greateſt: a very large thermometer, placed at a diſtance from the door of the room, but nearer to the wall than to the cockle, and defended from the immediate action of the cockle by a piece of paper hung before it, roſe one or two degrees above 260°: another thermometer, which had been ſuſpended very near the door, ſtood ſome degrees above 240°. At this time I went into the room, with the addition, to my common cloaths, of a pair of thick worſted ſtockings drawn over my ſhoes, and reaching ſome way above my knees; I alſo put on a pair of gloves, and held a cloth conſtantly between my face and the cockle: all theſe precautions were neceſſary to guard againſt the ſcorching of the red-hot iron. I remained eight minutes in this ſituation, frequently walking about to all the different parts of the room, but ſtanding ſtill moſt of the time in the cooleſt ſpot, near the loweſt thermometer. The air felt very hot, but ſtill by no means to ſuch a degree as to give pain: on the contrary, I had no doubt of being able to ſupport a much greater heat; and all the gentlemen preſent, who went into the room, were of the ſame opinion. I ſweated, but not very profuſely. For ſeven minutes my breathing continued perfectly good; but afte that time I began to feel an oppreſſion in my lungs, attended with a ſenſe of anxiety; which gradually increaſing for the ſpace of a minute, I thought it moſt prudent to put an end to the experiment, and immediately left the room. My pulſe, counted as ſoon as I came into the cool air, for the uneaſy feeling rendered me incapable of examining it in the room, was found to beat at the rate of 144 pulſations in a minute, which is more than double its ordinary quickneſs. To this circumſtance the oppreſſion on my breath muſt be partly imputed, the blood being forced into my lungs quicker than it could paſs through them; and hence it may very reaſonably be conjectured, that ſhould an heat of this kind ever be puſhed ſo far as to prove fatal, it will be found to have killed by an accumulation of blood in the lungs, or ſome other immediate effect of an accelerated circulation (b); for all the experiments ſhew, that heating the air does not make it unfit for reſpiration, communicating to it no noxious quality except a power of irritating. In the courſe of this experiment, and others of the ſame kind by ſeveral of the gentlemen preſent, ſome circumſtances occurred to us which had not been remarked before. The heat, as might have been expected, felt moſt intenſe when we were in motion; and, on the ſame principle, a blaſt of the heated air from a pair of bellows was ſcarcely to be born; the ſenſation in both theſe caſes exactly reſembled that felt in our noſtrils on inſpiration. The reaſon is obvious; when the ſame air remained for any time in contact with our bodies, part of its heat was deſtroyed, and conſequently, we came to be ſurrounded with a cooler medium than the common air of the room; whereas when freſh portions of the air were applied to our bodies in ſuch a quick ſucceſſion, that no part of it could remain in contact a ſufficient time to be cooled, we neceſſarily felt the full heat communicated by the ſtove. It was obſerved that our breath did not feel cool to the fingers unleſs they were held very near the mouth; at a diſtance the cooling power of the breath did not ſufficiently compenſate the effect of putting the air in motion, eſpecially when we breathed with force.

(a) See the former experiments, p. 111. of this volume.

(b) Since this experiment, I have obſerved the *mucus* from my lungs to be more *ſerous* than before, and to incline more to a ſaltiſh taſte, though the lungs themſelves ſeem perfectly ſound in all other reſpects; which raiſes a ſuſpicion that ſome of the ſmaller arteries ſuffered a degree of dilatation from the increaſed impulſe of the blood.

A chief object of this day's experiments was to aſcertain the real effect of our cloaths in enabling us to bear ſuch high degrees of heat. With this view I took off my coat, waiſtcoat, and ſhirt, and in that ſituation went into the room, as ſoon as the thermometer had riſen above the boiling point, with the precaution of holding a piece of cloth conſtantly between my body and the cockle, as the ſcorching was otherwiſe intolerable. The firſt impreſſion of the heated air on my naked body was much more diſagreeable than I had ever felt it through my cloaths; but in five or ſix minutes a profuſe ſweat broke out, which gave me inſtant relief, and took off all the extraordinary uneaſineſs: at the end of twelve minutes, when the thermometer had riſen almoſt to 220°, I left the room, very much fatigued, but no otherwiſe diſordered; my pulſe made 136 beats in a minute. On this occaſion I felt nothing of that oppreſſion on my breath which became ſo material a ſymptom in the experiment with my cloaths when the thermometer had riſen to 260°: this may be partly explained by the leſs quickneſs of my pulſe, the difference being at leaſt eight beats in a minute, and probably more, as in the experiment without my ſhirt the pulſations were counted before I had left the room; but there is a further circumſtance to be taken into conſideration, that the experiment attended with oppreſſion on the breath was made in the evening after a very plentiful meal, whereas the other was made in the forenoon, ſome hours after a moderate breakfaſt. The unuſual degree of fatigue which I felt from the experiment without my ſhirt, muſt be aſcribed in great meaſure to the more violent effort which the living powers were obliged to exert, in order to preſerve the due human temperature, when ſuch hot air came into immediate contact with my body. In the preſent caſe it appears beyond all doubt, that the living powers were very much aſſiſted by the perſpiration, that cooling evaporation which is a further proviſion of nature for enabling animals to ſupport great heats. Had we been provided with a proper balance, it would undoubtedly have rendered the experiment more complete to have taken the exact weight of my body at going into, and coming out of, the room; as from the quantity loſt ſome eſtimate might be formed of the ſhare which the perſpiration had in keeping the body cool; probably its effect was very conſiderable, but by no means ſufficient to account for the whole of the cooling, and certainly not equable enough to keep the temperature of the body to ſuch an exact pitch: For it ſhould here be remarked, that during all the experiments made this day, whenever I tried the heat of my body, the thermometer always came very nearly to the ſame point; I could not perceive even the ſmall difference of one degree, which was obſerved in our former experiments. Should theſe conſiderations, however, be thought inſufficient to prove that evaporation was not the ſole agent in keeping the body cool, I believe that Dr. FORDYCE's experiments in moiſt air will be found to remove all doubts on this ſubject. Several of the gentlemen preſent, as well as myſelf, went into the room without ſhirts many times afterwards, when the thermometer had riſen much higher, almoſt to 260°, and found that we could bear the heat very well, though the firſt ſenſation was always more diſagreeable than with our cloaths.

In all the experiments made this day it was obſerved, that the thermometer did not ſink ſo much in conſequence of our ſtay in the room as on the 23d of January; probably becauſe a much larger maſs of matter had been heated by the longer continuance of the fire.

Our own obſervations, together with thoſe of M. TILLET in the Memoirs of the Academy of Sciences(b), had given us good reaſon to ſuſpect, that there muſt have been ſome fallacy in the experiment with a dog, made at the deſire of Dr. BOERHAAVE, and related in his Elements of Chemiſtry(c). To determine this matter more exactly, we ſubjected a bitch weighing thirty-two pounds, to the following experiment. When the thermometer had riſen to 220°, the animal was ſhut up in the heated room, incloſed in a baſket, that its feet might be defended from

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(b) For the year 1764, p. 186, &c.

(c) Tom. I. p. 275.

the ſcorching of the floor, and with a piece of paper before its head and breaſt to intercept the direct heat of the cockle. In about ten minutes it began to pant and hold out its tongue, which ſymptoms continued till the end of the experiment, without ever becoming more violent than they are uſually obſerved in dogs after exerciſe in hot weather; and the animal was ſo little affected during the whole time, as to ſhew ſigns of pleaſure whenever we approached the baſket. After the experiment had continued half an hour, when the thermometer had riſen to 236°, we opened the baſket, and found the bottom of it very wet with *ſaliva*, but could perceive no particular *fætor*. We then applied a thermometer between the thigh and flank of the animal; in about a minute the quickſilver ſunk down to 110°: but the real heat of the body was certainly leſs than this, for we could neither keep the ball of the thermometer a ſufficient time in proper contact, nor prevent the hair, which felt ſenſibly hotter than the bare ſkin, from touching every part of the inſtrument. I have ſince found, that the thermometer held in the ſame place, when the animal is perfectly cool and at reſt, will not riſe above 101°. At the end of thirty-two minutes the bitch was permitted to go out of the room; upon coming into the cold air ſhe appeared perfectly briſk and lively, not in the leaſt injured by the heat, and has now continued very well above a month. Our experiment, therefore, differs, in every eſſential circumſtance of the event, from that related by Dr. BOERHAAVE. With reſpect to this laſt it is remarkable, if the facts be properly repreſented, that an intolerable ſtench aroſe from the dog; and that an aſſiſtant dropped down ſenſeleſs upon going into the ſtove.

To prove that there was no fallacy in the degree of heat ſhewn by the thermometer, but that the air which we breathed was capable of producing all the well-known effects of ſuch an heat on inanimate matter, we put ſome eggs and a beef-ſteak upon a tin frame, placed near the ſtandard thermometer, and farther diſtant from the cockle than from the wall of the room. In about twenty minutes the eggs were taken out, roaſted quite hard; and in forty-ſeven minutes the ſteak was not only dreſſed, but almoſt dry. Another beef-ſteak was rather overdone in thirty-three minutes. In the evening, when the heat was ſtill greater, we laid a third beef-ſteak in the ſame place: and as it had now been obſerved, that the effect of the heated air was much increaſed by putting it in motion, we blew upon the ſteak with a pair of bellows, which produced a viſible change on its ſurface, and ſeemed to haſten the dreſſing; the greateſt part of it was found pretty well done in thirteen minutes.

About the middle of the day two ſimilar earthen veſſels; one containing pure water, and the other an equal quantity of the ſame water with a bit of wax, were put upon a piece of wood in the heated room. In one hour and an half the pure water was heated to 140° of the thermometer, whilſt that with the wax had acquired an heat of 152°, part of the wax having melted and formed a film on the ſurface of the water, which prevented the evaporation. The pure water never came near the boiling point, but continued ſtationary above an hour at a much lower degree; a ſmall quantity of oil was then dropped into it, as had before been done to that with the wax; in conſequence of which, the water in both the veſſels came at length to boil very briſkly. A ſaturated ſolution of ſalt in water put into the room, was found to heat more quickly, and to an higher degree, than pure water, probably becauſe it evaporated leſs; but it could not be brought to boil till oil was added, by means of which it came toward evening into briſk ebullition, and conſequently had acquired an heat of 230°. Some rectified ſpirit of wine in a bottle ſlightly corked, which had been immerſed into this ſolution of ſalt whilſt cold, began to boil in about two hours, and ſoon afterwards was totally evaporated. Perhaps no experiments hitherto made furniſh more remarkable inſtances of the cooling effect of evaporation than theſe laſt facts; a power which appears to be much greater than hath commonly been ſuſpected. The evaporation itſelf, however, was more conſiderable in our experiments than it can be in almoſt any other ſituation, becauſe the air applied to the evaporating ſurface was uncommonly hot, and at the ſame time not more charged with moiſture than in its ordinary ſtate. A powerful aſſiſtant evaporation muſt undoubtedly prove, in keeping the living body properly cool, when expoſed to great heats; but it can act only in a *groſs* way, and by no means in ſuch a nice proportion to the momentary exigencies of the animal as would be requiſite for the exact preſervation of its temperature: that other proviſion of nature which ſeems more immediately connected with the powers of life, is, probably, the great agent in preſerving the juſt balance of temperature; exerting a greater effort in proportion as the evaporation is deficient, and a leſs effort as the evaporation increaſes. This idea correſponds with the general analogy of the animal oeconomy, the nicer balances of which are almoſt univerſally effected in that part of the body which is formed with the moſt ſubtile organization.

The heated room will, I hope, in time become a very uſeful inſtrument in the hands of the phyſician. Hitherto the neceſſary experiments have not been made to direct its application with a ſufficient degree of certainty. However, we can already perceive a foundation for ſome diſtinctions in the uſe of this uncommon remedy. Should the object in view be to produce a profuſe perſpiration, a dry heat acting on the naked body would moſt effectually anſwer that purpoſe. The hiſtories of dropſies and ſome other diſeaſes, ſuppoſed to have been cured by ſuch means, are well known to every phyſician. In ſome caſes alſo, a moiſt heat, and in others heat tranſmitted through a quantity of cloaths, might have their peculiar advantages. That the danger likely to enſue from ſuch applications is leſs than has been commonly apprehended, our former experiments gave ſufficient reaſon to believe, and the ſame was amply confirmed by thoſe which make the ſubject of this paper. For during the whole day, we paſſed out of the heated room, after every experiment, immediately into the cold air, without

out any precaution; after expoſing our naked bodies to the heat, and ſweating moſt violently, we inſtantly went out into a cold room, and ſtaid there even ſome minutes before we began to dreſs; yet no one received the leaſt injury. I felt nothing this day of the noiſe and giddineſs in my head, which had affected me in making the former experiments; and, whether from the force of habit, or any other cauſe, the ſhaking of our hands was leſs, and we felt leſs languor, though the heat had been ſo much more intenſe.

XLVIII. *A Propoſal for meaſuring the Attraction of ſome Hill in this Kingdom by Aſtronomical Obſervations. By the Rev.* Nevil Maſkelyne, *B. D. F. R. S. and Aſtronomer Royal.*

Redde in the year 1772.

IF the attraction of gravity be exerted, as Sir ISAAC NEWTON ſuppoſes, not only between the large bodies of the univerſe, but between the minuteſt particles of which theſe bodies are compoſed, or into which the mind can imagine them to be divided, acting univerſally according to that law, by which the force which carries on the celeſtial motions is regulated; namely, that the accelerative force of each particle of matter towards every other particle decreaſes as the ſquares of the diſtances increaſe, it will neceſſarily follow, that every hill muſt, by its attraction, alter the direction of gravitation in heavy bodies in its neighbourhood from what it would have been from the attraction of the earth alone, conſidered as bounded by a ſmooth and even ſurface. For, as the tendency of heavy bodies downwards perpendicular to the earth's ſurface is owing to the combined attraction of all the parts of the earth upon it, ſo a neighbouring mountain ought, though in a far leſs degree, to attract the heavy body towards its centre of attraction, which cannot be placed far from the middle of the mountain. Hence the plumb-line of a quadrant, or any other aſtronomical inſtrument, muſt be deflected from its proper ſituation by a ſmall quantity towards the mountain; and the apparent altitudes of the ſtars, taken with the inſtrument, will be altered accordingly.

It will eaſily be acknowledged, that to find a ſenſible attraction of any hill from undoubted experiment would be a matter of no ſmall curioſity, would greatly illuſtrate the general theory of gravity, and would make the univerſal gravitation of matter palpable, if I may ſo expreſs myſelf, to every perſon, and fit to convince thoſe who will yield their aſſent to nothing but downright experiment. Nor would its uſes end here; as it would ſerve to give us a better idea of the total maſs of the earth, and the proportional denſity of the matter near the ſurface compared with the mean denſity of the whole earth. The reſult of ſuch an uncommon experiment, which I ſhould hope would prove ſucceſsful, would doubtleſs do honour to the nation where it was made, and the ſociety which executed it.

Sir ISAAC NEWTON gives us the firſt hint of ſuch an attempt, in his popular Treatiſe of the Syſtem of the World, where he remarks, “That a mountain of an hemiſpherical figure, three miles high and ſix broad, will “not, by its attraction, draw the plumb-line two minutes “out of the perpendicular.” It will appear, by a very eaſy calculation, that ſuch a mountain would attract the plumb-line 1′ 18″ from the perpendicular.

But the firſt attempt of this kind was made by the French Academicians, who meaſured three degrees of the meridian near Quito in Peru, and who endeavoured to find the effect of the attraction of Chimboraço, a mountain in that neighbourhood, which is elevated near four miles above the ſea, though only about two miles above the general level of the province of Quito. By their obſervations of the altitudes of fixed ſtars taken with a quadrant of $2\frac{1}{2}$ feet radius, they found the quantity of 8″ in favour of the attraction of the mountain, by a mean of their obſervations. This, indeed, was much leſs than they expected; but then it is to be conſidered, that their inſtrument was too ſmall and imperfect for the purpoſe; and that they themſelves were ſubject to great inconveniencies, being ſheltered from the wind and weather by nothing but a common tent, and placed ſo high up the mountain as the boundary where the ſnow begins to lie unmelted all the year round. And indeed their obſervations, doubtleſs owing to theſe cauſes of error, differ greatly from one another, and are therefore inſufficient to prove the reality of an attraction of the mountain Chimboraço, although the general reſult from them is in favour of it. Accordingly, one of the French gentlemen themſelves, M. BOUGUER, who drew up the account of their experiment, expreſſes his wiſhes, that a like experiment might be made, to find the attraction of a mountain in France or England, where he thinks ſome might be found of ſufficient bulk for the purpoſe. This experiment and theſe remarks were made in the year 1738, or above thirty years ago, yet I believe no ſimilar experiment has ever been made in Europe.

I have made inquiries after a proper hill in this kingdom, for the trying of ſuch a curious experiment, and have been informed of two places in particular, extremely convenient for the purpoſe. The one is ſituated on the confines of Yorkſhire and Lancaſhire; where, within the compaſs of twenty miles, are ſituated four very remarkable hills, called Pendle-hill, Pennygant, Ingleborough, and Whernſide, which have been eſtimated to be from 600 to 750 yards elevated above the plane of the vales between them. By calculation on theſe *data*, it ſhould follow, that the ſum of the contrary attractions of Whernſide, the largeſt of theſe hills, on the plumb-line placed half-way up the hill, would not be leſs than 30″, and might amount to 46″, which it is evident is a very conſiderable quantity, and ſufficient to give us room to hope for a favourable and ſatisfactory ſucceſs of the experiment. The other place pointed out for this purpoſe, is a valley two miles broad, between the hills Helwellin and Skidda, in Cumberland; which hills, according to a plan of them and the adjacent country, communicated by Mr. SMEATON, F.R.S. are elevated above 1000 yards above the intermediate valley. By a calculation made according to this plan, the ſum of the contrary attractions of the plumb-line, placed alternately on the North-ſide of Helwellin and the South-ſide of Skidda, amounts to about 20″, which is likewiſe a quantity large enough for the experiment. And although the denſity of the earth near the ſurface ſhould be five times leſs than the mean denſity, as there is ſome reaſon to ſuſpect, and the attractions, as here ſtated, ſhould conſequently be diminiſhed in the proportion of five to one, ſtill the ſum of the contrary attractions of Whernſide would be $6''$ or $9''$, and the ſum of the contrary attractions of Helwellin and Skidda would be $4''$; which quantities are not too ſmall to be meaſured and demonſtrated by an accurate zenith ſector, ſuch as that belonging to the Royal Society, which I made uſe of at St. Helena, would be, if the fault in the ſuſpenſion of the plumb-line, which I there diſcovered, was corrected, in the manner ſuggeſted in the Philoſophical Tranſactions, vol. LIV. p. 351.

XLIX. An

XLIX. *An Account of Obſervations made on the Mountain* Schehallien *for finding its Attraction. By the Rev.* Nevil Maſkelyne, *B. D. F. R. S. and Aſtronomer Royal.*

Redde, July 6, 1775.

IN the year 1772, I preſented the foregoing propoſal, for meaſuring the attraction of ſome hill in this kingdom by aſtronomical obſervations, to the Royal Society; who, ever inclined to promote uſeful obſervations which may enlarge our views of nature, honoured it with their approbation. A committee was in conſequence appointed, of which number I was one, to conſider of a proper hill whereon to try the experiment, and to prepare every thing neceſſary for carrying the deſign into execution. The Society was already provided with a ten-feet zenith ſector made by Mr. SISSON, furniſhed with an achromatic object glaſs, the principal inſtrument requiſite for this experiment, the ſame which I took with me to St. Helena in the year 1761; which wanted nothing to make it an excellent inſtrument but to have the plumb-line made adjuſtable, ſo as to paſs before and biſect a fine point at the centre of the inſtrument. This was ordered to be done, and a new wooden ſtand provided for it, capable of procuring a motion of the ſector about a vertical axis, by means of which it could be more eaſily brought into the plane of the meridian,

ridian, or turned half round for repeating the obſervations with the plane of the inſtrument placed the contrary way, in order to find the error of the line of collimation. A large parallelopiped tent, $15\frac{1}{2}$ feet ſquare and 17 feet high, was alſo provided for ſheltering both the inſtrument and the obſerver who ſhould uſe it, compoſed of joices of wood well framed together, and covered with painted canvas. The Society was likewiſe poſſeſſed of moſt of the other inſtruments requiſite for this experiment; as an aſtronomical quadrant and tranſit inſtrument made by Mr. BIRD, and an aſtronomical clock by SHELTON, which had all been provided on occaſion of the obſervations of the tranſit of Venus in 1761 or 1769. A theodolite of the beſt ſort was wanting, a neceſſary inſtrument for obtaining the figure and dimenſions of the hill. One of Mr. RAMSDEN's conſtruction of 9 inches diameter, was thought the fitteſt for the purpoſe, on account of the excellence of the plan on which it was made, and the number of its adjuſtments, being capable of meaſuring angles for the moſt part to the exactneſs of a ſingle minute. The other inſtruments prepared for this buſineſs were, two barometers of M. DE LUC's conſtruction, made by Mr. NAIRNE; a common Gunter's chain; a roll of painted tape three poles long, having feet and inches marked upon it; two fir poles of 20 feet each, and four wooden ſtands, for ſupporting them when uſed in meaſuring the baſes, and a braſs ſtandard of five feet for adjuſting them. The poles and ſtands were provided on the ſpot.

Although accounts had been received from various perſons of ſeveral hills ſuppoſed proper for the intended purpoſe, ſome better and ſome worſe authenticated; yet, in order to be ſure of finding the beſt hill for the experiment, it was determined to ſend a perſon furniſhed with proper inſtruments, to make ſuch obſervations on various hills in England and Scotland, as might enable us to chooſe the fitteſt for the purpoſe. Accordingly Mr. CHARLES MASON, who had been employed on ſeveral aſtronomical occaſions by the Royal Society, was appointed to make a tour through the Highlands of Scotland in the ſummer of the year 1773, taking notice of the principal hills in England which lay in his route either in his going or in his return. It appeared from his obſervations, that ſcarce any hill was ſo well adapted to the purpoſe as our ſanguine hopes had led us to expect; for either they were not high enough, or not ſufficiently detached from other hills, or their greateſt length fell in a wrong direction, too near the meridian, inſtead of lying nearly Eaſt and Weſt, which is a circumſtance requiſite to make a hill of a given height afford the greateſt effect of attraction. In particular, the hills on the confines of Yorkſhire and Lancaſhire, mentioned in the foregoing propoſal, were found not to anſwer the deſcription that had been given of them. Fortunately, however, Perthſhire afforded us a remarkable hill, nearly in the centre of Scotland, of ſufficient height, tolerably detached from other hills, and conſiderably larger from Eaſt to Weſt than from North to South, called by the people of the low country Maiden-pap, but by the neighbouring inhabitants, Schehallien; which, I have ſince been informed, ſignifies in the Erſe language, Conſtant Storm: a name well adapted to the appearance which it ſo frequently exhibits to thoſe who live near it, by the clouds and miſts which uſually crown its ſummit. It had, moreover, the advantage, by its ſteepneſs, of having but a ſmall baſe from North to South; which circumſtance, at the ſame time that it increaſes the effect of attraction, brings the two ſtations on the North and South ſides of the hill, at which the ſum of the two contrary attractions is to be found by the experiment, nearer together; ſo that the neceſſary allowance of the number of ſeconds, for the difference of latitude due to the meaſured horizontal diſtance of the two ſtations in the direction of the meridian, would be very ſmall, and conſequently not ſubject to ſenſible error from any probable uncertainty of the length of a degree of latitude in this parallel. For theſe reaſons the mountain Schehallien was choſen, in preference to all others, for the ſcene of the intended operations; and it was concluded to make the experiment in the ſummer of the year 1774.

It was foreſeen, that this experiment would be attended with conſiderable expence, and ſuch as might eaſily have exceeded the common funds of the Royal Society, without ſome extraordinary aſſiſtance. The bounty of his Majeſty, our Patron, happily removed this difficulty. At the humble requeſt of the Society, his Majeſty had been graciouſly pleaſed to grant a very ample ſum to their diſpoſal, for defraying the expences of the obſervations of the late tranſit of Venus in 1769, as his Majeſty had before done with reſpect to the former tranſit of Venus in 1761. Out of this benefaction, after all expences had been paid, there was a conſiderable remainder, and, the Society humbly requeſting to know his Majeſty's pleaſure about the diſpoſal of it, he was graciouſly pleaſed to direct them, to *lay it out in ſuch manner as they thought proper, and was moſt agreeable to the end of their inſtitution.* As this bounty of his Majeſty had been originally granted for an aſtronomical purpoſe, the Society thought they could not diſpoſe of it on any more important object, or in any manner more conſiſtent with the intentions of their Royal Patron and Benefactor, than by expending it on this aſtronomical experiment of the attraction of a mountain, as what could hardly fail of throwing light on the principle of univerſal gravitation, and was likely to lead to new diſcoveries concerning the conſtitution of this earth which we inhabit, particularly with reſpect to the denſity of its internal parts.

The experiment being thus reſolved upon, the next next thing to be done was to fix on a proper perſon to carry it into execution. Numerous and intereſting as my literary engagements are at the Royal Obſervatory, I had no thoughts of undertaking this care and labour myſelf, till the Council of the Royal Society were pleaſed to do me the honour to think my aſſiſtance neceſſary to inſure the ſucceſs of ſo important and delicate an experiment. Their thinking ſo was a ſufficient motive with me to encounter whatever difficulties and fatigues might attend operations carried on in ſo inconvenient and inclement a ſituation. But it was requiſite I ſhould alſo have his Majeſty's permiſſion for abſenting myſelf ſo long from my duty at the Royal Obſervatory. This his Majeſty was graciouſly pleaſed to grant; and to allow me to ſtay as long as I thought neceſſary, to complete *my very important obſervations*.

Such were the motives for undertaking this experiment, and the preparations made for putting it in execution. I am now to give an account of the operations themſelves.

The quantity of attraction of the hill, the grand point to be determined, is meaſured by the deviation of the plumb-line from the perpendicular, occaſioned by the attraction of the hill, or by the angle contained between the actual perpendicular and that which would have obtained if the hill had been away. The meridian zenith diſtances of fixed ſtars, near the zenith, taken with a zenith ſector, being of all obſervations hitherto deviſed capable of the greateſt accuracy, ought by all means to be made uſe of on this occaſion: and it is evident, that the zenith inſtrument ſhould be placed directly to the North or South of the centre of the hill, or nearly ſo. In obſervations taken in this manner, the zenith diſtances of the ſtars, or the apparent latitude of the ſtation, will be found as they are affected by the attraction of the hill. If then we could by any means know what the zenith diſtances of the ſame ſtars, or what the latitude of the place would have been, if the hill had been away, we ſhould be able to decide upon the effect of attraction. This will be found, by repeating the obſervations of the ſtars at the Eaſt or Weſt end of the hill, where the attraction of the hill, acting in the direction of the prime vertical, hath no effect on the plumb-line in the direction of the meridian, nor conſequently on the apparent zenith diſtances of the ſtars; the differences of the zenith diſtances of the ſtars taken on the North or South ſide of the hill, and thoſe obſerved at the Eaſt or Weſt end of it, after allowing for the difference of latitude anſwering to the diſtance of the parallels of latitude paſſing through the two ſtations, will ſhew the quantity of the attraction at the North or South ſtation. But the experiment may be made to more advantage on a hill like Schehallion, which is ſteep both on the North and South ſides, by making the two obſervations of the ſtars on both ſides; for the plumb-line being attracted contrary ways at the two ſtations, the apparent zenith diſtances of ſtars will be affected contrary ways; thoſe which were increaſed at the one ſtation being diminiſhed at the other, and conſequently their difference will be affected by the ſum of the two contrary attractions of the hill. On the South ſide of the hill, the plumb-line being carried Northward at its lower extremity, will occaſion the apparent zenith, which is in the direction of the plumb-line continued backwards, to be carried Southward, and conſequently to approach the equator; and therefore, the latitude of the place will appear too ſmall by the quantity of the attraction; the diſtance of the equator from the zenith being equal to the latitude of the place. The contrary happens on the North ſide of the hill; the lower extremity of the plumb-line being there carried Southward will occaſion the apparent zenith to be carried Northward or from the equator; and the latitude of the place will appear too great by the quantity of the attraction. Thus the leſſer latitude appearing too ſmall by the attraction on the South ſide, and the greater latitude appearing too great by the attraction on the North ſide, the difference of the latitudes will appear too great by the ſum of the two contrary attractions; if therefore there is an attraction of the hill, the difference of latitude by the celeſtial obſervations ought to come out greater than what anſwers to the diſtance of the two ſtations meaſured trigonometrically according to the length of a degree of latitude in that parallel, and the obſerved difference of latitude ſubtracted from the difference of latitude inferred from the terreſtrial operations, will give the ſum of the two contrary attractions of the hill. To aſcertain the diſtance between the parallels of latitude paſſing through the two ſtations on contrary ſides of the hill, a baſe muſt be meaſured in ſome level ſpot near the hill, and connected with the two ſtations by a chain of triangles, the direction of whoſe ſides, with reſpect to the meridian, ſhould be ſettled by aſtronomical obſervations.

If it be required, as it ought to be, not only to know the attraction of the hill, but alſo from thence the proportion of the denſity of the matter of the hill to the mean denſity of the earth; then a ſurvey muſt be made of the hill to aſcertain its dimenſions and figure, from whence a calculation may be made, how much the hill ought to attract, if its denſity was equal to the mean denſity of the earth; it is evident, that the proportion of the actual attraction of the hill to that computed in this manner will be the proportion of the denſity of the hill to the mean denſity of the earth.

Thus there were three principal operations requiſite to be formed. 1. To find by celeſtial obſervations the apparent difference of latitude between the two ſtations choſen on the North and South ſides of the hill. 2. To find the diſtance between the parallels of latitude. 3. To determine the figure and dimenſions of the hill.

I arrived at the hill of Schehallien on the laſt day of June, and found the obſervatory and inſtruments there, which had been brought down ſome time before from London to Perth on board a ſhip, and thence conveyed over land to the hill under the care of Mr. REUBEN BURROW, my late aſſiſtant at the Royal Obſervatory. The obſervatory was fixed half-way up the South ſide of the hill, as the place where the effect of the hill's attraction would be at the greateſt, and it was placed in the like manner when it was afterwards removed to the North ſide. A circular wall was raiſed, five feet in diameter, and covered at top with a moveable conical roof for ſheltering the aſtronomical quadrant; and a ſquare tent was put up for receiving the tranſit inſtrument, all near to the obſervatory. A *bothie*, or temporary hut, was alſo made near it, for my reſidence, while I was attending the aſtronomical obſervations on this ſide of the hill. I firſt put the ſector, nearly in the meridian, by means of the variation compaſs; but, thro' the badneſs of the weather, which was almoſt continually cloudy or miſty, I could not before the middle of July get a ſufficient number of obſervations with the aſtronomical quadrant, to know the ſtate of the clock, in order to draw a meridian line on the floor of the obſervatory, for ſetting the ſector truly in the plane of the meridian. The firſt obſervations which I made with the ſector, after it was put truly in the meridian, were on the 20th of July. Between this time and the end of the month, I obſerved the zenith diſtances of 34 ſtars, ſome to the North and ſome to the South of the zenith; and many of them ſeveral times over, having taken 76 obſervations in all, with the plane of the ſector turned to the Eaſt. On the firſt of Auguſt, I turned the plane of the inſtrument about, to face the Weſt, and ſet it in the meridian again, by means of the meridian line drawn on the floor the 26th of July, and ſecured by picquets driven into the ground; and between that and the 15th of the ſame month, I obſerved 39 ſtars including moſt of thoſe taken in the former poſition of the inſtrument, and took 93 obſervations in all.

And here let me take notice of a method which I fell upon of verifying the poſition of the ſector, with reſpect to the plane of the meridian, which, had I thought of it at firſt, would have ſaved me much trouble; and therefore I will now mention it, as it may be uſeful to future obſervers. It conſiſts in obſerving the tranſits of two ſtars, differing conſiderably in declination from one another acroſs the vertical wire of the ſector, and comparing the obſerved difference of their tranſits with the known difference of their right aſcenſions. If they agree, it may be ſafely concluded, that the inſtrument is truly placed in the meridian. If not, by comparing the alteration that would be produced in the difference of the tranſits, by ſuppoſing the inſtrument out of the meridian, by any ſmall quantity, as one degree or ten minutes, with the obſerved error, the deviation of the inſtrument from the meridian may be inferred. In this manner I found, that the inſtrument had been put very exactly in the meridian by means of the meridian line; the difference by the two methods coming out only 2½ minutes of azimuth. As to the continuance of the inſtrument in the plane of the meridian, I had a conſtant proof of it by the ſame means, and likewiſe a farther ſecurity, which I did not fail to attend to, by noting the degree and minute which an index depending on the vertical axis of the inſtrument pointed out on a fixed azimuth circle. Being apprehenſive of error in an inſtrument ſupported on a wooden frame, I frequently examined the paralleliſm of the fore arch to the back arch, by meaſuring their perpendicular diſtances at the two ends with a braſs ſcale, whoſe vernier ſhewed the five hundredth part of an inch, and found it liable to variations of a minute or two, owing probably to the force uſed in ſetting the ſector to different zenith diſtances, and the weakneſs of ſome ſcrews at the top of the frame; which ſmall error I corrected, till I found it liable to continual returns: and I ſatisfied myſelf, that the plane of the ſector never deviated above three minutes from the meridian in any of the obſervations taken on the South ſide of the hill, which, it is evident, could not in the leaſt affect the obſerved zenith diſtances of ſtars. I hardly ever obſerved without examining the biſection of the point at the centre of the inſtrument, by the plumb-line; which was abſolutely neceſſary, on account of the gradual changes of the wooden frame. My view in mentioning theſe minute circumſtances is, to caution future obſervers, as well as to confirm my own obſervations. But whoever makes uſe of an inſtrument of this kind, ſupported on a wooden frame, will find the greateſt attention neceſſary to attain the ſame degree of accuracy in his obſervations, as if his inſtrument were fixed to an immoveable wall. In the mean time, by obſervations taken with the quadrant and tranſit inſtrument, I got a meridian line, and planted a pole to preſerve it on the top of the hill, to the South of the inſtrument, and another at the foot of the ſame hill; from whence, by meaſuring off an equal diſtance to the Eaſt (as the South-weſt corner of the obſervatory lay to the Eaſt of the tranſit inſtrument) and ſetting up another pole, another meridian line was got, paſſing through the South-weſt corner of the Southern ſtation of the obſervatory. The reaſon for making the meridian line paſs through the South-weſt corner of the obſervatory rather than through the middle of it was, that this part of it had been taken when

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the obſervatory had been uſed as an object in taking angles by the theodolite, in the ſurvey of the hill.

While I was engaged in theſe aſtronomical obſervations, Mr. BURROW, attended by Mr. WILLIAM MENZIES, a land-ſurveyor living in the neighbourhood, who had been recommended by ſome of the principal gentlemen of the country, as a proper perſon for this work, went out every day that the weather permitted, to take ſections of the hill, and angles between ſeveral objects, for determining the figure and dimenſions of the hill. The method made uſe of was this, which was propoſed by Mr. BURROW, and was well adapted to the purpoſe. A number of ſtation poles were ſet up at convenient diſtances all round the foot of Schehallien; but rather without its baſe, and chiefly on little eminences riſing from the foot of it, which formed a polygon of many ſides, ſurrounding the hill; and when delineated on paper, ſhew very nearly the ſhape of its baſe. At each ſtation, the angular poſition of two or more of the other ſtations being obſerved with the theodolite, and one ſide being determined by means of a meaſured baſe, all the other ſides will be known. From theſe ſtations, ſections of the hill up to the top were taken in the following manner. The theodolite, being placed at any ſtation, was pointed towards the hill; and a labourer was ſent with a number of poles, which he was to plant in the ground truly upright, at regular diſtances and in a vertical plane, according to ſignals which he received from the perſon that ſtood at the theodolite, who alſo took the altitude of the foot of each pole, and likewiſe the horizontal angle contained between the plane of the ſection poles and the next ſtation pole to the right or left. The theodolite was then removed, and planted directly over the centre of this ſtation pole, which was removed for this purpoſe; and the horizontal angle taken between a pole now planted at the firſt ſtation and each of the poles of the ſection. The horizontal diſtance of the two ſtation poles being known, the horizontal diſtance of each of the ſection poles from the firſt ſtation, and their reſpective perpendicular altitudes above it, or depth below it, will be given.

It is manifeſt, that theſe operations, when connected by angles with the two ſtations of the obſervatory and the meridian line, would at the ſame time give the ſhape and dimenſions of the hill, and the diſtance of the parallels of latitude paſſing through the two ſtations of the obſervatory, as well as their reſpective elevations above the baſe of the hill. But errors being apt to accumulate in a long chain of triangles; to obviate this danger, as well as to produce a check on any great miſtakes, that might happen to be made in reading off, or writing down, the angles, I cauſed a heap of ſtones, or *carn* as it is called by the people of the country, to be raiſed in a circular figure ſix feet high, at the higheſt point of the ridge of the hill, which is to the Weſt of it, as a ſignal to be obſerved from the ſeveral angles of the polygon, and as a means of connecting the two ſtations of the obſervatory by a ſmaller number of triangles. Another *carn* towards the Eaſtern end of the ridge of the hill was afterwards ſet up for the like purpoſe. I propoſed to determine the diſtance of the two *carns* by connecting them by angles with a baſe, to be meaſured in a level ſpot in the vale below the hill, and then to make uſe of the ſaid diſtance as a ſecondary baſe for determining the ſides of the polygon, and the diſtance of the two ſtations of the obſervatory. Had the two *carns* been viſible from the two ſtations of the obſervatory, two triangles would have ſufficed for connecting the two ſtations together. But, notwithſtanding that this was not the caſe, and that only the two *carns* were viſible from one another, yet all the angles of theſe two triangles were meaſured by Mr. BURROW in the following method, ſuggeſted by himſelf. He went with the theodolite to the neighbouring hill on the South ſide of Schehallien, which runs parallel to it; and, by varying his ſituation, found a point whence the Weſtern *carn* and Southern obſervatory appeared by the theodolite to be in one vertical plane, and removing the theodolite he planted a pole there. In like manner he planted another pole on the ſame hill, in a vertical plane with the Southern obſervatory and Eaſtern *carn*. Then returning to the obſervatory, he took the horizontal angle contained between the two poles, which it is evident is equal to its oppoſite angle, or that contained between the *carns*. And going to the Weſt *carn*, he took the angle contained between the Eaſt *carn* and the pole planted on the oppoſite hill, in a line with the Southern obſervatory and Weſt *carn*, which is the ſame with the angle between the Eaſt *carn* and Southern Obſervatory. And laſtly, going to the Eaſt *carn*, he took the angle contained between the Weſtern *carn* and the pole placed on the oppoſite hill in a line with the Eaſt *carn* and Southern obſervatory, which is the ſame with the angle contained between the Weſtern *carn* and Southern obſervatory. Thus were the three angles found of the triangle made by the Southern obſervatory and two *carns*. In the like manner were the angles of the triangle made by the Northern obſervatory and two *carns* found afterwards. And, as a proof that the angles of the two triangles were rightly determined, their ſum in the firſt caſe differed from 180° by little more than two minutes; and in the ſecond caſe by only half a minute.

Notwithſtanding the advantages which attended this method of finding the diſtance of the two ſtations of the obſervatory, I thought it proper to make uſe alſo of the other method of doing the ſame thing by a ſmall number of triangles carried directly acroſs the hill, thinking it expedient, in a matter of ſuch conſequence, to rely on no ſingle operation; but, as far as poſſible, to confirm every deduction by another found in an independent manner. I had cauſed two poles to be ſet as far up the hill of Schehallien as they could be placed; one as near the Weſtern, and the other the Eaſtern *carn* as they could be, ſo as to be viſible from the Southern ſtation of the obſervatory: alſo two others in like manner viſible from the North obſervatory; one of which was very near the Eaſt *carn*, and the other only 269 feet diſtant from the Weſternmoſt of the two poles viſible from the South obſervatory; ſo narrow was the ridge of the hill in that part, although it grew wider both to the Weſt and

Eaſt, but much more towards the latter. With theſe four poles, the Eaſt *carn*, and the two ſtations of the obſervatory, five triangles were formed, connecting the two ſtations of the obſervatory, the relative ſituation of which to each other would be determined as ſoon as the length of any one of the ſides of theſe triangles was known, either by comparing it with a baſe meaſured in the valley below, or with the diſtance of the two *carns* ſettled in that manner.

I had got ſufficient obſervations of zenith diſtances of ſtars with the ſector on the South ſide of the hill by the 15th of Auguſt; I prepared therefore for removing the obſervatory and inſtruments to the new ſtation on the North ſide. This was a work of great labour and difficulty, as every thing was carried over the ridge of the hill on men's ſhoulders, and ſome of the packages were very weighty; it employed the labour of twelve men for a week, and was compleated on the 26th. A large level area had been cut away, with great labour, here, in the ſide of the hill, for receiving the obſervatory, as had before been done on the South ſide of the hill. A new *bothie* was alſo erected, and places for holding the quadrant and tranſit inſtrument, as before, adjoining to the obſervatory.

The badneſs of the weather prevented me from beginning my obſervations with the ſector till the 4th of September; but, that being a clear night, I had a fair opportunity of putting in practice the method of bringing the inſtrument into the meridian by the tranſits of the ſtars acroſs the plane of the ſector, which was mentioned before. The ſector being put up with its plane facing the Weſt, and ſet near the meridian by the variation compaſs, allowing for the variation, I found, by the tranſit of o Draconis, on the North ſide of the zenith, compared with thoſe of $\varkappa$, ι and θ Cygni on the South ſide, that the inſtrument deviated $49\frac{1}{2}$ minutes to the Weſt of the South in azimuth; which being corrected, by turning the inſtrument about on its vertical axis, towards the Eaſt, by the help of the diviſions on the azimuth circle; I then found, by the tranſit of η Cephei, on the North ſide of the zenith compared with that of π Cygni on the South ſide, that the inſtrument deviated ſeven minutes to the Eaſt of the South in azimuth, which I corrected accordingly. And ſo near was it brought to the meridian in this manner, that by the moſt exact compariſon of the tranſits of ſeveral ſtars on the 7th and 8th inſtant, it appeared to be only two minutes out of the meridian, and that to the Eaſt of the South; which ſmall error I alſo attempted to correct; but the inſtrument reſted one minute out of the poſition which I intended to give it, owing to the difficulty of turning it about to ſuch great nicety, and ſo I let it remain.

It was indeed a moſt fortunate circumſtance, that I thus got the inſtrument ſo near the meridian by the very firſt night's obſervations, thoſe of September 4th; for the badneſs of the weather in the day prevented me from getting a meridian line by the Sun till the 15th. Had I therefore been obliged to wait for ſetting the inſtrument right by the Sun, I ſhould

I ſhould have loſt four good days obſervations, which were two-thirds of thoſe I took on this ſide of the hill with the plane of the inſtrument turned to the Weſt, and been retarded near three weeks in my obſervations; and, as the opportunities of weather fit for obſerving at all were but very rare, I might have been thereby thrown back into the winter, and defeated of making ſo complete a ſet of obſervations on the North ſide of the hill as I had got on the South ſide, whoſe correſpondence would thereby have been rendered leſs perfect. I had the ſatisfaction, however, when I drew the meridian line on the floor of the obſervatory by the equal altitudes of the Sun taken on the 15th, to find it agree perfectly, even to the ſame minute, with the poſition of the inſtrument, as determined by the tranſits of the ſtars. But no one will doubt of the ſuperior eaſe and readineſs afforded by the latter method, in preference to the other.

On the 20th of September I compleated the obſervations with the plane of the ſector turned to the Weſt, having obſerved 32 ſtars, and taken 68 obſervations in all. On the 22d, I turned it about with the plane to face the Eaſt, and ſet it again in the meridian, by putting it parallel to its former poſition, by means of the meridian line ſecured by marks made on picquets let into the ground perpendicularly below the plane of the inſtrument, before it was turned. Between this time and the 24th of October, I obſerved 37 ſtars, and took 100 obſervations in all, with the plane of the inſtrument facing the eaſt:

and thus I compleated my whole ſeries of obſervations with the ſector, having obſerved 43 different ſtars in all, on both ſides of the hill, and taken 337 obſervations.

As a few obſervations, taken with ſo excellent an inſtrument as this zenith ſector, would have been ſufficient to determine the apparent difference of latitude of the two ſtations of the obſervatory, to a ſecond or two; I am apprehenſive I may be thought by many to have multiplied obſervations unneceſſarily. However that may be, I apprehend, that doubling the obſervations in each ſtation of the obſervatory, by taking them with the plane of the inſtrument alternately facing the Eaſt and Weſt, will be allowed to be a proper ſtep, as the line of collimation of the inſtrument is hereby ſeparately determined at each ſtation, and thereby all danger of any alteration happening in the ſame, in its removal from one ſide of the hill to the other, is intirely obviated. I had, indeed, all the reaſon in the world to think, that the ſector was carried from one ſtation to the other without the leaſt accident; but ſtill it was proper to guard againſt what was poſſible to happen.

But I had reaſons alſo for multiplying the obſervations made in the ſame poſition of the inſtrument. It was important to demonſtrate the exactneſs of the inſtrument from the near agreement of a number of obſervations taken with it, as its excellence was not to be intirely preſumed, unleſs this proof could be ſhewn in its favour. Beſides, it might be expected, that ſome unſteadineſs or warping of the wooden ſtand on which it was ſupported, might affect the accuracy of the obſervations; or there might be variable and diſcordant refractions, even near the zenith, on the ſide of ſo ſteep a hill, more than are found in lower ſituations. Add to this, that when I began my obſervations on the South ſide of the hill, having a proſpect of bad weather before me, and not knowing how few obſervations I might be able to get on either ſide of the hill, I thought it prudent to endeavour to obſerve moſt of the ſtars in the Britiſh catalogue, which came within the reach of the inſtrument, that I might be ſure of being provided with obſervations of ſome at leaſt of the ſame ſtars, which I might afterwards obſerve when I ſhould be removed to the North ſide of the hill; where, after an interval of perhaps ſome months, many ſtars, that before paſſed the meridian in the night, would paſs it in the day, and conſequently be either inviſible through the teleſcope of the ſector, or more precarious of being ſeen.

Although a meridian line had been found by the tranſit inſtrument at the South obſervatory, whereby the relative ſituation of the two ſtations of the obſervatory, as well as of the other points of the hill, with reſpect to the meridian, might be determined; yet I judged it would be more ſatisfactory to confirm this by another meridian line drawn at the Northern obſervatory. This I found, as I had done the former, by ſetting the tranſit inſtrument to agree with the pole-ſtar at the computed time of its paſſing the meridian, and confirmed it by comparing the difference of the tranſits of the pole-ſtar and of α Pegaſi, α Andromedæ, and γ Pegaſi, with their difference of right aſcenſion, in the ſame manner by which I had put the ſector in the plane of the meridian, and found it to agree with the former meridian line within two minutes.

It remains to give an account of the manner in which the two baſes were meaſured; one in a level ſpot at the foot of the hill, to the Southward; and the other at the diſtance of about 2½ miles from the hill to the North-weſt, in the plain of Rannock. I cauſed two meaſuring poles to be made of ſtrait-grained well-ſeaſoned fir, in the form of ſquare tubes, 3 inches ſquare and 20 feet long, and ſtrengthened with ſquare pieces withinſide at ſeveral diſtances. Theſe were carefully compared with the braſs ſtandard made by Mr. BIRD, the ſame which was uſed in the meaſure of the degree at Penſylvania, immediately before they were applied to the meaſure of the baſes, and the height of the thermometer noted at the time in order to make allowance for the expanſion or contraction of the braſs ſtandard by heat or cold. Four wooden ſtands were provided for ſupporting the poles; each having a triangular baſe with three iron ſpikes beneath, at each of the angles. An upright pole, ſix feet high, roſe from the middle of one ſide of the triangle, and two ſhort braces were joined to it from the two ends of this ſide, and a long ſlant pole from the oppoſite angle. Two ſliding arms were put upon the upright pole, capable of being raiſed or depreſſed, one above and the other below the place where the ſlant pole was faſtened to the upright pole, for ſupporting the meaſuring poles at a convenient height above the ground. In meaſuring the baſe, one end of a pole was ſupported on one of the ſtands, and the other end on another ſtand; and it was ſet horizontal by means of a ſpirit level laid on it about the middle, and by raiſing or depreſſing the arm on which it reſted at one or the other end. The other pole was then, in like manner, ſupported on the two other ſtands truly level, and in the ſame vertical plane with the former pole, namely, that of the intended baſe, without regarding whether they were exactly of the ſame height, and with ſome ſmall horizontal interval between their ends. This interval was meaſured by laying one leg of a braſs rectangle, which was divided into inches and tenths, along one pole, while the other, or vertical leg, touched the end of the other pole: for it was not thought adviſeable, to bring the ends of the poles to touch exactly, as that would have taken up a great deal of time, and might have endangered the altering the poſition of the hindermoſt pole, if it ſhould chance to receive any ſhock by laying down the foremoſt pole. It is evident, that the inches and tenths given by the diviſions of the braſs rectangle are to be added into one ſum together with the poles, in computing the length of the baſe. When the foremoſt pole was truly placed, and the interval between them had been meaſured by the divided ſide of the braſs rectangle, the hindermoſt pole was taken up, and the ſtands on which it had reſted were advanced forwards, and the pole again laid on them, truly level, and in the true direction of the baſe. In order to ſet the poles continually in the proper direction of the baſe, the following method was uſed. The theodolite was firſt ſet up at one end of the baſe, and an upright pole at the other, and another in the middle, and a third was from time to time advanced to a little diſtance forward; and the meaſuring poles were ſometimes placed in the proper direction by the eye, looking along the lengths of both poles together to the upright pole before them, and ſometimes by the help of the theodolite. In this manner, about the middle of September, a baſe was meaſured by Mr. BURROW and Mr. MENZIES of 3012 feet, in the valley at the foot of the hill to the South-weſt; but not ſo accurately as this method is capable of, owing to the ſtands being very unſteady, through the looſeneſs of the ſpikes in the feet and other faults, during the meaſuring the firſt quarter of the baſe, though they were mended before the menſuration of the remainder of it. The menſuration of another baſe of the length of 5897 feet, in the meadow of Rannock, about 2½ miles to the South-weſt of the centre of the hill, which I attended myſelf, was performed with the greateſt accuracy, according to the ſame method, on the 10th, 11th, and 12th of October, with new ſtands, more ſubſtantial and firm than the former.

The extreme badneſs of the weather no leſs retarded the operations of the ſurvey than the celeſtial obſervations; for there was almoſt conſtant rain, miſt, or high wind, to obſtruct the uſe of the theodolite; indeed all the people of the country agreed, it was the worſt ſeaſon that had ever been known. So that it was not till the 20th of October that the ſections had been carried all round the hill. Nor would this work have been ſo much forwarded as it was, had it not been for the uſe of an additional theodolite of the ſame conſtruction, and by the ſame maker, as the former, which was lent me, upon my requeſt, by the right honourable JAMES STUART MACKENZIE, lord privy ſeal for Scotland; who, having long cultivated a diſtinguiſhed taſte for aſtronomy, was pleaſed to honour the experiment of attraction with every aſſiſtance, which his intereſt or recommendation could procure. I am particularly to acknowledge the favour he conferred upon me by introducing me to the acquaintance of Sir ROBERT MENZIES, baronet, his brother-in-law, a gentleman converſant in mathematical and philoſophical learning, who honoured me with his friendſhip during my reſidence in the country; and, beſides many perſonal civilities ſhewn to myſelf, rendered many material aſſiſtances to the main purpoſe of carrying on the experiment. It is with pleaſure alſo, that I acknowledge the civilities of all the neighbouring gentlemen, who often paid me viſits on the hill, and gave me the fulleſt conviction that their country is with juſtice celebrated for its hoſpitality and attention to ſtrangers. I was honoured alſo by viſits from many learned gentlemen who came from a great diſtance; particularly the lord privy ſeal, Dr. WILSON, profeſſor of aſtronomy at Glaſgow, and his ſon, and Dr. REID, profeſſor of moral philoſophy, and Mr. ANDERSON, profeſſor of natural philoſophy, alſo at GLASGOW, lord POLWARTH, Mr. RAMSAY, profeſſor of natural hiſtory at Edinburgh, Mr. Commiſſioner MENZIES of the Cuſtoms at Edinburgh, Mr. COPLAND and Mr. PLAYFORE, of the univerſity of Aberdeen, the rev. Mr. BRICE, and my eſteemed friend Col. ROY, who had been my companion in the journey as far as Edinburgh. So great a noiſe had the attempt of this uncommon experiment made in the country, and ſo many friends did it meet with intereſted in the ſucceſs of it!

The uſe of the two theodolites at once, as mentioned above, much forwarded the compleating of the ſections all the month of October; Mr. MENZIES obſerving the bearings at one ſtation with one theodolite, while Mr. BURROW obſerved the altitudes or depreſſions with the other theodolite at the other ſtation; and the labourer, who uſed to plant the poles in the hill, taking only one pole with him, and fixing it up at one place to be obſerved at both theodolites, and then removing it to the next ſtation for the like purpoſe. Notwithſtanding which, the weather grew at length ſo bad, by the early coming in of froſt and ſnow in the beginning of November, when the ſurvey was near compleated, as to render it impoſſible to do any thing more that ſeaſon. It became therefore neceſſary to finiſh this aſtronomical campaign, leaving the theodolite in the care of Mr. MENZIES, to complete what little remained to be done the next ſeaſon.

I have thus deſcribed the plan which was adopted for the operations on Schehallien, and the manner in which it was carried into execution; it only remains to give the reſult of computations made upon thoſe operations for deducing the effect of the attraction of the mountain. The operations themſelves at large ſhall be communicated another opportunity.

I had cauſed the arch of the ſector to be divided by fine points, according to a new and arbitrary diviſion adapted to the method of continual biſection. One-eighth part of the radius of the inſtrument was found by three biſections, and applied as a chord to the arch from the middle on each ſide, intercepting each way 7° 9′ 59″,917. Theſe ſpaces were each divided by points into 128 parts, by continual biſection; therefore one diviſion will contain 3′ 21″,561854. Hence the number of degrees and minutes anſwering to any number of diviſions may eaſily be found. Twenty-four additional parts were alſo ſet off taken from the former, and added at the 128th diviſion on each ſide, to fill up the whole extent of the arch, which thus conſiſted of 152 diviſions on each ſide of the centre, anſwering to an angle of 8° 30′ 37″,4, which was therefore the greateſt angle the inſtrument was capable of meaſuring. To find the value of the parts of the micrometer in ſeconds, I meaſured the diſtance of the points on the limb, by five at a time, by means of the plumb-line, in parts of the micrometer from 0 to 128 on each ſide of the middle or point marked 0 upon the arch. By a mean of all theſe meaſures, one diviſion of the arch or $3' \, 21'',562$ came out equal to four revolutions and 34,8272 parts of the micrometer, 41 of which make one revolution; and therefore one part is equal to $1'',0137545$, and 41 or one revolution is equal to $41'',5639345$. Hence the value of any number of revolutions and parts of the micrometer may be eaſily found. At all obſervations of the ſame ſtar, whether on the North or South ſide of the hill, I brought the ſame point of the arch, namely, that which agreed neareſt with the zenith diſtance of the ſtar, under the plumb-line, ſo that the difference of the apparent zenith diſtances of the ſame ſtar on contrary ſides of the hill is given in parts of the micrometer, and has no reference to the diviſions of the inſtrument, whether they be equal or unequal; and, the parts of the micrometer ſcrew being perfectly equal, as I had formerly ſatisfied myſelf by meaſuring the interval of two given points on the arch with different parts of the ſcrew, that difference of apparent zenith diſtances may be perfectly relied on, as far as depends upon the inſtrument, provided the biſection of the ſtar by the wire in the teleſcope, and that of the point on the arch by the plumb-line, were accurately performed. As the plane of the inſtrument was placed both Eaſt and Weſt, at both ſtations of the obſervatory, the difference of the latitude of the two ſtations may be found as well from the obſervations made in one poſition of the inſtrument as the other. If the inſtrument had ſuffered no change by being carried over the hill, that is to ſay, if the line of collimation was not altered thereby, the reſults ſhould come out equally true from the obſervations taken in both poſitions of the inſtrument. On the contrary, if the line of collimation ſhould, by any means, have ſuffered any alteration between the obſervations made at the two ſtations, this would cauſe the difference of latitude to appear too ſmall, by the obſervations made in one poſition of the inſtrument, by the quantity of the alteration, and as much too great, in the other poſition of the inſtrument. But ſtill the mean between the two reſults, deduced from the obſervations taken in the two different poſitions of the inſtrument, would give the true difference of latitude; and that equally, whether the line of collimation had ſuffered any change or not. Therefore this will be the beſt method of comparing the obſervations together, and I ſhall take a mean of all the reſults, deduced from the obſervations taken in each poſition of the inſtrument ſeparately, and then a mean of thoſe means for the true difference of latitude. By ſingle obſervations of ten ſtars; *viz.* β, α, and η Caſſiepeæ, and ι, η, β, 39, 45, 46, and 53 Draconis, made on both ſides of the hill, with the plane of the ſector facing the Weſt, after making the proper allowance for preceſſion, aberration, and deviation, and ſemi-annual ſolar nutation of the earth's axis (ſee my tables annexed to my Obſervations made at the Royal Obſervatory), the apparent difference of latitude between the two ſtations of the obſervatory, comes out 54″,1, 54″,7, 54″,0, 55″,4, 55″,0, 55″,0, 52″,2, 54″,0, 54″,3, 53″,1, reſpectively; the mean of all which is 54″,2; the greateſt difference of any one reſult from the mean being only 2″. In like manner, by ſingle obſervations of as many ſtars; *viz.* β and α Caſſiepeæ; ε Urſæ majoris; β, 39, 46, o, 49, and 53, Draconis; and 23 Cygni; made on both ſides of the hill, with the plane of the ſector facing the Eaſt; after making all the allowances as before, the apparent difference of latitude comes out 54″,5, 52″,3, 56″,8, 53″,5, 54″,5, 57″,2, 56″,1, 55″,3, 54″,1, 55″,1, reſpectively; the mean of all which is 55″; the greateſt difference of any one reſult from the mean being 2″. The two means 54″,2 and 55″,0 differ only 0″,8, which ſhould argue only an alteration of 0″,4 in the line of collimation; but this is too ſmall a quantity to be depended on; and therefore it is moſt probable, that the ſtate of the inſtrument remained unvaried. However, whether it did or no, the mean of the two means, or 54″,6, is to be eſteemed the apparent difference of latitude between the two ſtations of the obſervatory, and, when compared with the difference of latitude which ſhould reſult from the trigonometrical meaſures, will give the ſum of the two contrary attractions of the hill.

It muſt be owned, that this point will be ſettled with rather more certainty when all the obſervations made with the ſector, which exceed 300, ſhall have been computed; but, as from the agreement of theſe reſults together, as well as from the ſmall differences that are uſually found in obſervations made within a few days of one another, one may preſume, that the reſult from the whole will not differ materially from that deduced above from 40 obſervations, I thought I had better take this opportunity of gratifying the impatience of the Society in preſenting them with theſe my firſt computations before their ſummer receſs, than delay giving them any account at all of this experiment, till I had leiſure to complete the whole of my calculations.

I am now to ſhew, what the diſtance is between the parallels of latitude paſſing through the two ſtations of the obſervatory in feet, according to the trinogometrical menſuration; and thence, what the difference of latitude ought to have been, if the hill had been away, or had exerted no ſenſible attraction. This depends on the enumeration of ſeveral particulars.

The length of the baſe meaſured in the meadow of Rannoch was 5897,119 feet, according to the ſtate of the braſs ſtandard when the thermometer was at 40°; but, to reduce it to anſwer to the ſtate of the braſs ſtandard in the heat of 62°, we muſt ſubtract 16,721 feet; we ſhould alſo ſubtract farther 0,327, for the diminution which the braſs ſtandard has ſuffered by wear, and there remains 5880,071 feet for the true length of the baſe in Rannoch. See Phil. Tranſ. vol. LVIII. p. 313. 324. and 326. Hence, with the help of the angles taken with the theodolite at the ends of the baſe in Rannoch, and at the Weſt *carn*, the horizontal diſtance between the Eaſt and Weſt *carns* comes out 4047,4 feet. Nearly the ſame reſult comes out from the baſe meaſured on the South ſide of the hill, though with leſs exactneſs; this, when all corrections are made, is 3011,684 feet, whence the diſtance of the two *carns* ſhould come out 4058 feet, or about ten feet longer than reſults from the baſe in Rannoch. But I prefer the deduction from the baſe in Rannoch as moſt to be depended on. Hence, by the calculation of the two triangles formed by the two *carns* and the two ſtations of the obſervatory, the diſtance between the parallels of latitude paſſing through the two ſtations comes out 4364,4 feet, which, according to M. BOUGUER's table of the length of a degree in this latitude of 56° 40′, at the rate of 101,64 Engliſh feet to one ſecond, anſwers to an arc of the meridian of 42″,94. The other ſeries of triangles carried acroſs the hill, gives the ſame diſtance of the parallels only 10 feet leſs, and conſequently the arc of the meridian only $\frac{1}{10}$th of a ſecond leſs. Thus the difference of latitude found by the aſtronomical obſervations, comes out greater than the difference of latitude anſwering to the diſtance of the parallels, the former being 54″,6, the latter only 42″,94. The difference 11″,6 is to be attributed to the ſum of the two contrary attractions of the hill.

The attraction of the hill, computed in a rough manner, on ſuppoſition of its denſity being equal to the mean denſity of the earth, and the force of attraction being inverſely as the ſquares of the diſtances, comes out about double this. Whence it ſhould follow, that the denſity of the hill is about half the mean denſity of the earth. But this point cannot be properly ſettled till the figure and dimenſions of the hill have been calculated from the ſurvey, and thence the attraction of the hill, found from the calculation of ſeveral ſeparate parts of it, into which it is to be divided, which will be a work of much time and labour; the reſult of which, will be communicated at ſome future opportunity.

Having thus come to a happy end of this experiment, we may now conſider ſeveral conſequences flowing from it, tending to illuſtrate ſome important queſtions in natural philoſophy.

1. It appears from this experiment, that the mountain Schehallien exerts a ſenſible attraction; therefore, from the rules of philoſophiſing, we are to conclude, that every mountain, and indeed every particle of the earth, is endued with the ſame property, in proportion to its quantity of matter.

2. The law of the variation of this force, in the inverſe *ratio* of the ſquares of the diſtances, as laid down by Sir ISAAC NEWTON, is alſo confirmed by this experiment. For, if the force of attraction of the hill had been only to that of the earth, as the matter in the hill to that of the earth, and had not been greatly increaſed by the near ap-

proach to its centre, the attraction thereof muſt have been wholly inſenſible. But now, by only ſuppoſing the mean denſity of the earth to be double to that of the hill, which ſeems very probable from other conſiderations, the attraction of the hill will be reconciled to the general law of the variation of attraction in the inverſe duplicate *ratio* of the diſtances, as deduced by Sir ISAAC NEWTON from the compariſon of the motion of the heavenly bodies with the force of gravity at the ſurface of the earth; and the analogy of nature will be preſerved.

3. We may now, therefore, be allowed to admit this law; and to acknowledge, that the mean denſity of the earth is at leaſt double of that at the ſurface, and conſequently, that the denſity of the internal parts of the earth is much greater than near the ſurface. Hence alſo, the whole quantity of matter in the earth will be at leaſt as great again as if it had been all compoſed of matter of the ſame denſity with that at the ſurface; or will be about four or five times as great as if it were all compoſed of water. The idea thus afforded us, from this experiment, of the great denſity of the internal parts of the earth, is totally contrary to the hypotheſis of ſome naturaliſts, who ſuppoſe the earth to be only a great hollow ſhell of matter; ſupporting itſelf from the property of an arch, with an immenſe vacuity in the midſt of it. But, were that the caſe, the attraction of mountains, and even ſmaller inequalities in the earth's ſurface, would be very great, contrary to experiment, and would affect the

meaſures of the degrees of the meridian much more than we find they do; and the variation of gravity in different latitudes in going from the equator to the poles, as found by pendulums, would not be near ſo regular as it has been found by experiment to be.

4. The denſity of the ſuperficial parts of the earth, being, however, ſufficient to produce ſenſible deflections in the plumb-lines of aſtronomical inſtruments, will thereby cauſe apparent inequalities in the menſurations of degrees in the meridian; and therefore it becomes a matter of great importance to chuſe thoſe places for meaſuring degrees, where the irregular attractions of the elevated parts may be ſmall, or in ſome meaſure compenſate one another; or elſe it will be neceſſary to make allowance for their effects, which cannot but be a work of great difficulty, and perhaps liable to great uncertainty.

After all, it is to be wiſhed, that other experiments of the like kind with this were made in various places, attended with different circumſtances. We ſeldom acquire full ſatisfaction from a ſingle experiment on any ſubject. Some may doubt, whether the denſity of the matter near the ſurface of the earth may not be ſubject to conſiderable variation; though perhaps, taking large maſſes together, the denſity may be more uniform than is commonly imagined, except in hills that have been volcanos. The mountain Schehallien, however, bears not any appearance of having ever been in that ſtate; it being extremely ſolid and denſe, and ſeemingly compoſed of an intire rock. New obſervations on the attraction of other hills, would tend to procure us ſatisfaction in theſe points. But whatever experiments of this kind be made hereafter, let it be always gratefully remembered, that the world is indebted for the firſt ſatisfactory one to the learned zeal of the Royal Society, ſupported by the munificence of GEORGE THE THIRD.

Obſervations of Stars made with the zenith ſector on the mountain Schehallien, for diſcovering its attraction.

		Reſult of Obſervations on the South-ſide of Schehallien.										
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the Eaſt.					Limb of the Sector turned to the Weſt.					
α Caſſiopeæ	24S	July 28. S 1.35,4::					Aug. 4. S 2.7,1	7. 5,5				
η	1S						Aug. 7. N 10,4	10. 12,1	15. 12,7			
β Urſæ Major.	16N						Aug. 8. N 2.23,1					
γ	31S	July 29. N 2.16,9					Aug. 8. N 2.5,7 :					
ε	9N	July 28 N 2.23,6										
ζ	10S	July 28. N 1.11,5										
η	111S	July 22. N 19,8	28. 22,6									
ι Draconis	55N	July 26. N 2.26,0	28. 26,0				Aug. 4. N 2.14,0					
θ	45N	July 26. N 4,2					Aug. 4. S 7,5					
η	96N	July 25. N 6,2	26. 5,4				Aug. 2. S 6,5	4. 5,4				
β	75S	July 21. N 1.37,4	24. 37,0	25. 36,1			Aug. 1. N 1.27,4	2. 24,7 :	4. 24,4	5. 26,9	6. 26,4	15. 28,7
ξ	4N	July 20. N 3.4,5	21. 4,3	25. 5,4	26. 6,4		Aug. 1. S 9,2	15. 7,9				
γ	92S	July 20. N 1.32,0:	25. 32,3				Aug. 5. N 1.20,7					
39	36N	July 24. N 19,1	26. 19,5	29. 19,8			Aug. 1. N 7,9	4. 11,3	15. 11,1			

Reſult of Obſervations on the South-ſide of Schehallien.

Names of the Stars.	Point on the limb.	Limb of the Sector turned to the Eaſt.					Limb of the Sector turned to the Weſt.				
45 Draconis.	4N	July 21. N 1,0	29. 2,8				Aug. 1. S 9,4	4. 11,2	15. 5,6		
46	24S	July 24. N 23,9	26. 24,7				Aug. 4. N 13,6	10. 16,4:	15. 16,6		
ο	44N	July 21. S 4,2	25. 2,7	26. 2,2	28. 2,0	29. 1,0	Aug. 1. S 12,7	4. 12,4			
48	15N						Aug. 11 N 2.5,1	15. 6,0			
49	24S	July 29. N 2.28,0					Aug. 1. N 2.16,4	4. 17,6			
53	3S	July 26. S 22,2	28. 21,5				Aug. 1. S 32,0	4. 31,5	15. 28,4		
54	12N	July 29. S 18,4					Aug. 15. S 25,5				
κ Cygni	66S	July 21. S 0,1	26. N 0,3								
ι	97S	July 21. N 2.36,0	28. 35,0	29. 35,8	31. 36,5		Aug. 1. N 2.23,5	4. 22,8			
θ	124S	July 26. S 19,6	31. 17,5				Aug. 1. S 31,4				
23	5N	July 24. N 8,5	29. 8,7	31. 11,0			Aug. 1. S 2,7	4. 2,4	7. 1,5	15. N 1,1	
33	14S	July 24. N 1.0,2	26. 0,5	29. 1,3	31. 3,1		Aug. 1. N 30,4	4. 30,7	7. 31,6	15. 34,9	
η Cephei	77N	July 23. S 5,5	26. 2,4	29. 1,2	31. 1,6		Aug. 1. S 12,5	4. 11,2	7. 10,5	10. 9,8	15. 7,5
α	89N	July 23. S 25,0	26. 22,0	28. 20,8	29. 21,0	31. 20,0	Aug. 1. S 33,9	4. 32,8	7. 32,8	10. 31,7	15. 28,5
π Cygni	116S	July 29. N 12,8					Aug. 7. 0,0:	10. N 1,9			

		Reſult of Obſervations on the South-ſide of Schehallien.										
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the Eaſt.					Limb of the Sector turned to the Weſt.					
10 Cephei	61N						Aug. 4. N 28,8					
12	44N						Aug. 12. N 21,9					
13 μ	20S	July 26. N 34,0	29. 34,2				Aug. 7. N 24,7 :	10. 27,1	15. 28,3			
14	4N	July 26. N 2.38,5					Aug. 4. N 2.27,7	10 31,5				
15 ν	37N						Aug. 12. S 24,3					
21 ζ	8N	July 29. S 1.4,7	31. 2,9				Aug. 4. S 1 14,6	10. 11,5	15 11,0			
	13S	July 23. S 27,8	26. 26,7				Aug. 4. S 30,0	10. 33,8				
27 δ	11N	July 26. S 37,0	30. 36,9				Aug. 4. S 1.6,7	7. 6,7	9. 6,0	10. 5,5	15. 3,3	
ι	149N						Aug. 9. N 1.3,5					
1 Caſſiopeæ	27N	July 29. N 2.29,9	31. 30,3				Aug. 4. N 2.20,7	15. 22,9				
2	26N	July 31. S 13,0					Aug. 4. S 24,9	9. 23,5	15. 21,4			
5 τ	13N						Aug. 10. N 16,2	15. 17,5				
7 ρ	8S						Aug. 10. S 2.14,4	15. 13,6				
β	22N	July 26. N 39,2					Aug. 4. N 29,0	7. 29,5	15. 31,9			

		Reſult of Obſervations on the North-ſide of Schehallien.									
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the Weſt.				Limb of the Sector turned to the Eaſt.					
α Caſſiopeæ.	24S	Sept. 8. S 3.9,0	15. 7,4			Oct. 2. S 2.27,5:	3. 25,3	5. 26,8	24. 22,3		
η	1S	Sept. 7. S 33,8	8. 33,5	19. 30,3		Oct. 2. S 13,4	3. 11,8	9. 11,0	24. 7,2		
β Urſæ Major.	16N					Oct. 6. N 1.3,0					
γ	31S					Oct. 14. N 29,0	15. 25,5	18. 26,5	22. 26,4		
ε	9N					Oct. 3. N 35,5	4. 35,6	15. 30,0	17. 33,1		
ζ	10S	Sept. 15. S 24,2				Oct. 16. S 19,6	22. 22,0				
η	111S					Oct. 17. S 1.6,3	18. 6,6	22. 6,9			
ι Draconis	55N	Sept. 20, N 37,6				Oct. 17. N 1.6,1	22. 1,4				
θ	45N					Oct. 17. S 1.13,7	23. 16,4				
η	96N	Sept. 7. S 1.18,0	20. 19,4			Oct. 23. S 1.12,3					
β	75S	Sept. 7. N 17,3	15. 17,5			Oct. 7. N 28,8	17. 27,2	23. 26,1			
ξ	4N	Sept. 15. N 1.29,5				Oct. 23. N 1.39,9					
γ	92S	Sept. 18. N 13,6	20. 13,4								
39	36N	Sept. 7. S 36,8	20. 38,9			Oct. 3. S 25,4	17. 26,5				
45	4N	Sept. 7. S 1.14,0				Oct. 17. S 1.1,1	23. 2,0				

		Reſult of Obſervations on the North-ſide of Schehallien.									
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the Weſt.				Limb of the Sector turned to the Eaſt.					
46 Draconis.	24S	Sept. 7. S 33,0				Oct. 3. S 20,9	17. 18,7	23. 19,4			
ο	44N	Sept. 18. S 1.16,5				Oct. 3. S 1.4,8	18. 3,3	22. 3,9			
49	24S	Sept. 20 N 1 12,2				Oct. 3. N 1.25,5	22. 26,5:	23. 26,3			
53	3S	Sept. 7. S 1.35,0				Oct. 3. S 1.21,6	17. 20,0				
54	12N					Oct. 17. S 1.17,5	22. 18,0				
κ Cygni	66S					Oct. 18. S 40,0					
ι	97S	Sept. 7. N 1.20,0	18. 20,7	20. 20,5		Oct. 3. N 1.36,0	15. 35,9	18. 36,7			
θ	124S	Sept. 7. S 1.34,6	20 33,7			Oct. 6. S 1.17,0:	22. 17,6:				
23	5N	Sept. 7. S 1.5,9	18. 3,9	20. 3,0		Oct. 3. S 29,3	15. 28,3	18. 28,6			
33	14S	Sept. 7. S 13,1	18. 10,3	20. 10,8		Oct. 3. N 4,0	6. 5,8	7. 5,8			
η Cephei	77N	Sept. 7. S 1.12,5	8. 13,1	18. 9,3	20. 10,6	Oct. 3. S 36,9	6. 35,1	7. 35,9			
α	89N	Sept. 7. S 1.33,6	8. 35,0	20. 31,9		Oct. 3. S 1.13,5	6. 12,0	23. 9,7			
π Cygni	116S	Sept. 7. S 1.1,1	8. 1,4			Oct. 6. S 21,5	9. 21,4				
10 Cephei	61N	Sept. 20. S 10,0									
13 [illegible]	20S	Sept. 7. S 17,5	18. 14,9			Oct. 3. N 1,5	7. 1,7	9. 3,5			

		Reſult of Obſervations on the North-ſide of Schehallien.									
Names of the Stars	Point on the limb.	Limb of the Sector turned to the Weſt.				Limb of the Sector turned to the Eaſt.					
14 Cephei	4N					Oct. 9. N 2.9,8					
21 ζ	8N	Sept. 7. S 2.16,0	18. 11,7	20. 11,2:		Oct. 3. S 1.35.6	6. 35,3	9. 33,5			
ε	13S	Sept. 7. S 1.38,0	8. 37,3			Oct. 3. S 1.17,5	9. 15,0	23. 13,3			
27 δ	11N	Sept. 8. S 2.8,0				Oct. 3. S 1.30,6	6. 26,5	9. 25,5	22. 22,6	23. 22,6	24. 23,4
ι	149N	Sept. 8. N 1,7	15. 3,8								
1 Caſſiopeæ.	27N	Sept. 7. N 1.17,4	13. 21,2	15. 20,4		Oct. 3. N 1.39,0	6. 39,9:	9. 2.0,0			
2	26N	Sept. 7. S 1.27,0	8. 26,0	15. 23,5		Oct. 3. S 1.4,5	6. 5,4	9. 3,9			
5 τ	13N	Sept. 7. S 29,4	8. 28,1	13. 26,4		Oct. 2. S 7,8	3. 7,8	9. 5,1			
7 ρ	8S	Sept. 7. S 3.18,7	8. 18,5	15. 18,3		Oct. 3. N 1.38,0	6. 36,0	9. 40,5	24 2.4,6		
β	22N	Sept. 7. S 14,0	8. 13,6	13. 11,7	15. 11,7	Oct. 3. N 6,9	6. 7,9	9. 8,9			

N. B. The figures ſet down under the days of the month are the revolutions and parts of the micrometer, by which the ſtar is North or South of the diviſion ſhewn in the ſecond column. The revolution, though not repeated, is ſuppoſed to continue the ſame as in the firſt ob-

obſervation of the ſtar. One part of the micrometer is $= 1'',0137545$, and one revolution or 41 parts is $= 41'',56393$. 128 diviſions of the arc have the chord $= \frac{1}{8}$th of the radius.

PRESENTS

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In the YEAR 1774;

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267.	2.	*for* caves *read* eaves
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A

DISCOURSE

ON THE

Attraction of Mountains,

DELIVERED AT THE

Anniverſary Meeting of the ROYAL SOCIETY,
November 30, 1775.

By Sir JOHN PRINGLE, Baronet,
PRESIDENT.

PUBLISHED BY THEIR ORDER.

LONDON:
Printed for the ROYAL SOCIETY.

M.DCC.LXXV.

GENTLEMEN,

THE ſatisfaction you diſcovered when a propoſal was laid before you, *for meaſuring the attraction of mountains*, and the manner in which you received the account of what had been done to fulfil that view, were ſuch indications of your applauſe, that your Council, ever attentive to your ſentiments, have adjudged the prize-medal of this year to the Reverend *Nevil Maſkelyne*, his Majeſty's Aſtronomer at Greenwich, the author and conductor of that experiment. The many and valuable communications of our worthy brother, preceding this inquiry, you have never failed to diſtinguiſh; but theſe his late labours, undertaken at your requeſt, with their ſucceſsful reſult, related in his Paper, intitled *Obſervations made on the Mountain Schehallien for finding its Attraction*, and inſerted in the ſecond part of the volume of your Tranſactions for this year, ſeemed to lay the

Society under ſuch obligations, as your Council preſumed you could not otherwiſe expreſs than by the higheſt mark of your approbation. In conſequence of this reflection, I have by their authority cauſed Mr. *Maſkelyne's* name, with the date of the preſent year, to be engraven on the medal, in order to perpetuate to him the honour you were this day to confer upon him; if, after allowing me to recall to your remembrance ſome of the more intereſting particulars of this diſquiſition and his operations, you ſhould not refuſe your ſanction to the judgment of your Council.

I ſhall not conſider the ſubject of attraction at large, nor touch upon any ſpecies of it, excepting what in latter times, by the effects, has been diſtinguiſhed by the name of *gravity* or *gravitation*; a property of bodies perceptible to the vulgar when things fall to the ground, but long acknowledged by this Society, to be a quality impreſſed by the Creator on all matter, whether of the earth or of the heavens, whether at reſt or in motion: *He commanded, and it was created.*

The diſcovery of this extenſive principle, the phyſics of aſtronomy, depended upon a juſt notion of the arrangement and motions of the ſpheres; for to underſtand their œconomy, it was neceſſary previouſly to know, which of the ſtars were quieſcent, which moved, and in what manner. Whoever therefore found out the true celeſtial ſyſtem, might be ſaid to have paved the way to the knowledge of that ſublime truth, the law by which the natural world is governed. But who were the inventors here? Were they Chaldeans or Ægyptians? Was it *Pythagoras*, or *Philolaus*, or any other Greek, either in their own country, or tranſplanted to the mathematical ſchools of Alexandria? I ſhall not enter upon that inquiry, as fruitleſs as obſcure. All that is clear and to our purpoſe is, that ſome of the ancient Greeks conjectured rightly about the ſtability of the ſun and the circular motion of the earth; but this was never a general perſuaſion, nor does it ſeem to have been mentioned any more after the age of *Ptolemy*, who in the ſecond century did not ſo much invent a new ſyſtem, as adopt that which now goes under his name, the prevailing one of his time, and nearly the ſame with that of *Ariſtotle*. This, though erroneous, was not perhaps incapable of improvements from celeſtial obſervations; but when the philoſophy of the ſchools was united with the Ptolemaic hypotheſis, and both were ſubjected to judicial aſtrology, then was aſtronomy debaſed to the level of the pretended learning of the dark ages that enſued, and increaſed their darkneſs.

But at the appointed time, when it pleaſed the ſupreme Diſpenſer of every good gift to reſtore light to a bewildered world, and more particularly to manifeſt his wiſdom in the ſimplicity as well as in the grandeur of his works, he opened the glorious ſcene with the revival of a ſound aſtronomy. *Copernicus* of Thorn (a Poliſh city in the Regal Pruſſia) endowed by nature with excellent talents, improved by a ſuperior degree of mathematics, and by travelling, became early in life diſguſted with the contradictions about the cauſes of the celeſtial phænomena. He had recourſe, as he himſelf informs us *, to every author upon the ſubject, to ſee whether any had been more conſiſtent in explaining the irregular motions of the ſtars, than the mathematical ſchools; but received no ſatisfaction, till firſt, from *Cicero*, he found that *Nicetas* had maintained the motion of the earth; and next from *Plutarch*, that others of the ancients had been of the ſame opinion. *Cicero* had ſaid, that Nicetas *the Syracuſian (according to* Theophraſtus)

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* Præf. ad lib. de Revolutionibus Orbium Cœleſtium.

phraſtus) *held that the heavens, the ſun, the moon, the ſtars, in a word, the whole celeſtial bodies ſtood ſtill, and that, excepting the earth, nothing moved in the world; but that whilſt the earth with the greateſt celerity turned round its axis, the ſame phænomena were produced, as if it ſtood ſtill, and the heavens moved. And this ſome thought was alſo* Plato's *notion, but ſomewhat obſcurely expreſſed* *.

Plutarch's words were, *Others ſuppoſe the earth to be at reſt; but* Philolaus *the Pythagorean, that it is carried in the ecliptic round the fire, like the ſun and the moon.* Heraclides *of Pontus, and* Ecphantus *the Pythagorean, make the earth move like a wheel about its center from Weſt to Eaſt, but not to change its place* †.

From theſe quotations, and what *Copernicus* farther ſays ‡, we find how little diſpoſed that great man was to plume himſelf with the inventions of others; nay, rather anxious not only to do juſtice to thoſe who had gone before him, but by their authority to ſcreen himſelf from the cenſure of innovation, abſurdity, and impiety, that awaited the publication of his doctrine. After all, the original genius of *Copernicus* was but little beholden for the diſcovery of thoſe ſublime truths, to

* Cicer. Quæſt. Academic. † Placit. Philoſ. lib. 3. cap. 3. ‡ Ibid.

either *Nicetas* or *Plato*, ſince it appears from *Cicero* that theſe two believed both the moon and the planets to be motionleſs. Nor could he be more aſſiſted by *Philolaus*, who taught that *the earth turned round* a *fire*; but this fire could not be the ſun, becauſe that ancient compares the motion of the earth about *the fire*, to the revolution of the ſun and moon about the earth. Laſtly, what little light *Copernicus* could draw from *Heraclides* and *Ecphantus*, I ſcarcely need ſay, ſince they, though admitting the diurnal motion of the earth, denied the annual.

But if *Copernicus* ſought to do juſtice, why did he not rather cite a clear and expreſs paſſage in the *Arenarius* of *Archimedes* for the fixed ſtate of the ſun, and for the motion of the earth in a circle round his body? *What moſt philoſophers call the world* (ſays that famous ancient) *is a ſphere, of which the center is that of the earth, and whereof the ſemi-diameter is equal to a right line joining the centers of the earth and the ſun. But* Ariſtarchus *the Samian, refuting this opinion, has advanced an hypotheſis, whereby the world ſhould be many times greater than what is here ſaid; for he ſuppoſes that the fixed ſtars and the ſun remain immoveable, and that the earth is* *carried in a circle round the ſun, placed in the middle of it's courſe* *.

Thus far *Archimedes*, who ſeems not to diſapprove the ſyſtem, but who explains it no farther, as what he had quoted was ſufficient for his purpoſe. It is probable that the penetrating genius of *Ariſtarchus* had diſcovered the true arrangement of the whole celeſtial bodies, and thereby totally anticipated *Copernicus*; but that circumſtance is no where, that I know of, recorded; and otherwiſe, we ſhould acquit our illuſtrious reformer of plagiariſm with regard to *Ariſtarchus*, ſince neither the *Arenarius* of *Archimedes*, where that paſſage is found, nor indeed any other of his valuable remains, had ſeen the light before the death of *Copernicus*. This extraordinary perſon had even before the meridian of life completed his diſcoveries, and compriſed them in his book *de Revolutionibus Orbium Cœleſtium*, his only work; but which he had prudently ſuppreſſed, till he had maturely conſidered his ſubject, and had found a neceſſary and powerful patron, the pope himſelf, *Paul* III, a lover of aſtronomy, to protect him. Alluding to the admonition of the poet, he tells the Pontif, *he had*

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* Archimed. Arenar. ed. Oxon. 1676.

ſuffered that fruit of his labours to ripen, not nine years only, but four times nine *. Conſenting at laſt to the publication, he committed the care of the impreſſion to ſome friends in a diſtant city, from whom he received the finiſhed copy a few hours before he expired †.

Few compoſitions have deſtroyed more riveted errors, or eſtabliſhed more important truths. Here, inſtead of an abſolute ſtate of reſt for the earth, it's triple motion is aſcertained, the diurnal about it's axis, the annual about the ſun, and that other known by the term *preceſſion of the equinoxes*; all which till then had been referred to the motion of the heavens. He likewiſe demonſtrated the double orbit of the moon, that is, her menſtrual motion about the earth, and her annual about the ſun. Nor did the wiſe *Copernicus* ſtop here; for, after laying this ſolid foundation of the celeſtial phyſics, he began the ſuperſtructure, by ſurmiſing a principle of *attraction* to be inherent in all matter. Thus, in refuting the peripatetic notion, that bodies fall to the ground, becauſe by a law of nature every thing heavy tends to the center of the univerſe (which they ſuppoſed to be in the center of the earth) he obſerved, that *the earth could not be the center of the orbits of ſeveral of the*

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* Præfat. ad lib. de Revolut. † Gaſſend. in vita Copernic.

planets, becauſe of the apparent irregularities of their motions, and therefore could not be the center of the univerſe: hence, according to theſe philoſophers, there muſt be more centers than one; and if ſo, who could tell the true center, toward which all bodies were to gravitate? As for gravity, ſays he, *I conſider it as nothing more than a certain natural appetence* (appetentia) *that the Creator has impreſſed upon all the parts of matter, in order to their uniting and coaleſcing into a globular form, for their better preſervation; and it is credible that the ſame power is alſo inherent in the ſun and moon and planets, that thoſe bodies likewiſe may conſtantly retain that round figure in which we behold them* *. Farther, *Copernicus* looked upon the ſun as the chief governing power of the earth and all the other planets; for after placing the great luminary in the center, he cries out with rapture, *Profecto tanquam in ſolio regali ſol reſidens circumagentem gubernat aſtrorum familiam* †. Nor was this *government* underſtood to be exerciſed by any other power than that of *attraction*; as may be inferred from ſome of the laſt words of the celebrated *Tycho Brahe*, who perceiving the approach of death, called for the famous *Kepler* (then a young man, and his aſſiſtant in his obſervatory

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* De Revolut Orb. Cœleſt. lib. 1. cap. 9. † Ibid. cap. 10.

at Prague) and after charging him with completing and publiſhing the aſtronomical tables which he was leaving unfiniſhed, thus addreſſed him: *My friend, although what I aſcribe to a voluntary, and as it were an obſequious motion of the planets round the ſun, you attribute to an* attractive *energy of that body; yet I muſt entreat you, that in the publication of my obſervations, you would explain all the celeſtial motions by my hypotheſis, rather than by that of* Copernicus, *which I know you would otherwiſe incline to follow*[*].

From this paſſage, which I have taken from the life of *Tycho Brahe*, it would ſeem, that though that other excellent aſtronomer was not inſenſible of ſome influencing power of the ſun over the planets, he would not however expreſs it by ſo ſtrong a term as *attraction*. But in what manner *Kepler* complied with the requeſt of his dying patron, it is not to our preſent purpoſe to mention, and therefore we ſhall only obſerve, that in his own works he conſtantly maintains the doctrine of attraction, and carries it even farther than ever *Copernicus* had done. Thus he calls gravity *a corporeal and mutual affection between ſimilar bodies, in order to their union*[†]. Again he remarks with *Copernicus*, againſt the peripatetics, that

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[*] Gaſſend. in Vit. Tych. Brah. cap. 5.

[†] Aſtron. Nov. in Introduct.

heavy bodies do not tend to the center of the univerſe, but to the center of thoſe larger round bodies of which they make a part; ſo that if the earth were not ſpherical, things would not fall from all points towards its center. If a ſtone were to be placed at a diſtance from another ſtone, in any part of the univerſe, without the ſphere of action of a third body, like two magnets, they would come together in ſome intermediate point, each advancing, in ſpace, in the inverſe proportion of their quantities of matter. Hence if the moon and the earth were not by ſome power kept aſunder in their reſpective orbits, they would move towards one another; the moon making 53 parts of the way while the earth made one, ſuppoſing their denſities equal[*].

From the ſame principle *Kepler* accounted for the general motion of the tides, to wit, by the attraction of the moon, and expreſly calls it *virtus tractoria quæ in luna eſt*[†]. He adds, that if the earth did not exert an attractive power over its own waters, they would riſe and ruſh to the moon[‡]. Farther, we find him ſuſpecting certain irregularities in the motion

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[*] Aſtron. Nov. in Introduct.

[†] Ibid.

[‡] Ibid.

of the moon to be owing to the combined action of the earth and ſun upon it's body[*]. Theſe, and other reflections concerning the univerſality of attraction, he accompanies with an ingenious anticipation of a law of nature, from conjecture only, but which was afterwards made out by experiments. The ſchools had taught, that *ſome bodies were by their nature heavy, and ſo fell to the ground, and that others were by their nature light, and therefore mounted upwards:* but *Kepler* pronounced, that *no bodies whatſoever were abſolutely light, but only relatively ſo; and conſequently, that all matter was ſubjected to the law of gravitation*[†].

Hitherto the genius of *Kepler* had been fortunate, in tracing out that great principle which hindered the planets from flying off from the ſun: but what kept them from falling into that maſs of fire, and what power perpetuated their motion in their orbits? Here his ſagacity failed him, and left his imagination to furniſh the idea of a ſyſtem of *vortices* for *Deſcartes*.

But howſoever incomplete theſe notions were concerning gravitation, yet in juſtice to their diſtinguiſhed authors, *Copernicus* and *Kepler*, I thought proper to commemorate them on this occaſion, as none before them had expreſſed themſelves ſo fully, and with ſo much truth on that curious ſubject; and as none, from their days, to thoſe of Dr. *Hooke*, made any ſuch improvement, as would apologize for my taking up ſo much more of your time in recalling their ſentiments to your remembrance. Let it ſuffice to mention, that the firſt who in this country embraced that doctrine was Dr. *Gilbert* *; but who did not properly diſtinguiſh between attraction and magnetiſm; and that the next was lord *Bacon*, who, though not a convert to the *Copernican* ſyſtem, yet acknowledged an attractive power in matter †. In France we find *Fermat* and *Roberval*, mathematicians of great eminence, of the ſame opinion ‡; and in Italy *Borelli*, after *Galileo* ||, who was the firſt in that country who conceived that idea, but far from that preciſion and extenſion we find it in his contemporaries *Bacon* and *Kepler*.

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[*] Aſtron. Nov. cap. 37.

[†] Ibid. in Introduct.

Before we paſs from *Kepler*, it will be proper to obſerve, that this great improver of aſtronomy did not, perhaps, after all, contribute ſo much to the advancement of this theory, by thoſe conjectures which I have related, as by ſome aſtronomical deductions from *Tycho Brahe's* obſervations, ſince known by the name of *Kepler's Laws*. The firſt was, that the planets move not in circular, but in elliptical orbits, of a ſmall eccentricity, whereof the center of the ſun makes one of it's *foci*. The ſecond, that the ſame planet deſcribes about the ſun equal areas in equal times. The third, that in different planets, the ſquares of the periodic times are as the cubes of their mean diſtances from the ſun.

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* De Magnete. † Nov. Organ. lib. 2. aphor. 36, 45, 48. Sylv. Sylvar. cent. 1. exp. 33. ‡ Montucla Hiſt. des Mathem. part. 4. liv. 8. || Syſt. Coſmic.

Such were the preparatives to the true philoſophy, and indeed excellent materials for the architect then unborn. But till ſir *Iſaac Newton* appeared, notwithſtanding the numerous and momentous diſcoveries that had been made in the heavens, by *Copernicus*, *Tycho Brahe*, *Galileo*, *Kepler*, and others, yet aſtronomy, as lord *Bacon* complained, ſtill remained but a mathematical ſtudy. The paſſage to which I allude is long, but, as tending to illuſtrate more than one particular relating to my ſubject, I cannot forbear treſpaſſing on your indulgence by the citation. *Although aſtronomy* (ſays *Bacon*) *has not been founded amiſs upon obſervation of the phænomena, yet the ſuperſtructure has hitherto kept low and weakly.*

In truth that ſcience preſents to the human underſtanding ſuch an object as Prometheus *did of old to* Jupiter, *when, meaning to impoſe upon that deity, he offered upon his altar, inſtead of a live victim, the hide of a large bullock, ſtuffed with ſtraw, leaves and oſier branches. In like manner aſtronomy exhibits the externals of the celeſtial bodies, as the cuticular part of heaven, fair indeed and artificially formed into a ſyſtem; but the entrails and the fountains of life are wanting, that is, the phyſical cauſes and reaſons; from which and from aſtronomical hypotheſes, a theory ſhould be drawn, not adequate only to account for all the phænomena; but for the ſubſtance, the motion, and influx of the heavens as they are in nature.... Scarcely is there one to be found who has inquired into the natural cauſes, either of the ſubſtance of celeſtial matter, or into the reaſon of the ſwiftneſs or ſlowneſs of the heavenly bodies, acting upon one another; or into the various degrees of motion of the ſame planet, or into the motion from Eaſt to Weſt, or of the contrary direction; nor into the progreſſions, ſtations, and retrogradations of thoſe bodies.... Nor into the cauſes of the apogæum and perigæum.... I ſay, inquiries of this kind have ſcarcely been attempted, nor indeed any labour beſtowed upon the ſubject, excepting in the way of mathematical obſervations and demonſtrations. So that aſtronomy,*

aſtronomy, ſuch as it now is, can only be reckoned among the mathematical arts; not without conſiderable diminution of its dignity; ſince were it to maintain its rights, it might rank itſelf as the nobleſt branch of philoſophy. For he that ſhall reject the fictitious divorces between the ſuperlunary and ſublunary bodies, and ſhall duely attend to the appetences and moſt general affections of matter (which both in the earth and in the heavens are exceedingly powerful, and indeed pervade the univerſe) will receive from what he ſees paſſing on the earth clear information concerning the nature of celeſtial bodies: and contrariwiſe, from motions which he ſhall diſcover in the heavens, will learn many particulars relating to the things below, that now lie concealed from us. Wherefore the phyſical part of aſtronomy we mark as wanting, and call it the aſtronomia viva, *the animated aſtronomy, in oppoſition to the ſtuffed bullock of* Prometheus *.

The great *deſideratum* was ſupplied, and from the boſom of this Society, in the publication of the *Principia*, the immortal work of *Newton*. There the illuſtrious author evinces truths that had been only ſurmiſed before; and after eſtabliſhing by a juſt analyſis the laws of attraction, in a ſynthetical method proceeds to explain by them the motions and appearances of the heavenly bodies. Had not *Newton* lived, *Bacon* might have paſſed for a viſionary ſpeculator; but ſince the demands of that noble author upon the human intellects have been ſo fully anſwered in the productions of ſir *Iſaac Newton*, ſhall we not revere thoſe powers of his own mind, that could, in that dawn of philoſophy in which he lived, ſo well deſcry what parts were wanting, and what were the means of attaining them?

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* De Dign. & Augm. Scient. l. 3. c. 4.

Newton in a poſthumous treatiſe, *de Syſtemate Mundi*, compoſed before the publication of the *Principia*, and mentioned there, has ſaid, that *ſome of the latter philoſophers had ſought to account for the courſe of the planets in their orbits by the action of certain* vortices, *as* Kepler *and* Deſcartes; *or by ſome other principle of impulſe or attraction, as* Borelli, Hooke, *and others of our nation.* From this paſſage it would ſeem, that in thoſe times there had been more conjectures formed concerning attraction, than what were publiſhed; for excepting *Gilbert*, who vainly attempted to explain the mundane ſyſtem by magnetiſm, and lord *Bacon*, who never acceded to the *Copernican* hypotheſis *, I have found none of our nation,

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* Atque harum ſuppoſitionum abſurditas, in motum terræ diurnum (*quod nobis conſtat falſiſſimum eſſe*) homines impegit. Bac. de Dign. & Augm. Scient. lib. 3. cap. 4.

tion, *Hooke* excepted, who in this way have left any thing on record worthy of your notice. He indeed, the early, the ingenious and moſt uſeful member of this Society, advanced in this reſearch far beyond all that had gone before him. But I ſhall not enlarge upon his improvements, as you have in your hands his *Cutlerian Lectures*, which contain them, and as I have already but too long dwelt on this part of my ſubject. It will ever redound to the praiſe of *Hooke*, that *Newton* has aſſociated him with himſelf in maintaining the true regulating cauſe of the courſe of the planets *. As to *Borelli*, though I have found in one of the pieces (a ſcarce one) of that learned Italian, a paſſage that certainly favours attraction; yet as it is neither ſo full, nor ſo explicit upon that point, as ſeveral others which I have cited, I muſt ſuſpect that thoſe parts which *Sir Iſaac* had in his eye have eſcaped my obſervation †.

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* M. Montucla has done great juſtice to Dr. *Hooke*, in this and other particulars, in his excellent work, *Hiſt. de Mathem. part.* 4. *liv.* 8.

† This is the paſſage alluded to: *Præterea manifeſtum eſt, quemlibet ſive primarium, ſive ſecundarium planetam aliquem inſignem mundi globum quaſi virtutis fontem circumdare, qui ita eos ſtringit, atque conglutinat, ut ab ipſo nullo pacto abſtrahi poſſint; ſed ipſum, quacunque contendentem, perpetuis continuiſque orbibus cogantur conſequi: videmus enim Saturnum, Jovem, Martem, Venerem, atque Mercurium Solem ipſum, Medicæa Sidera Jovem, Hugenianumque Sidus Saturnum circumire, non ſecus, ac circa Telluris Globum Luna ipſa revolvitur. Joa. Alph. Borelli Theor. Medic. Planetar. ex cauſis Phyſicis deductæ*, lib. 1. cap. 2. p. 5. Florent. 1666. 4°.

The great completer of the doctrine of univerſal gravitation had the ſatisfaction to find, from the reception it met with in this Society, that he had not laboured in vain; nay, perhaps no philoſophical author was ever more admired and followed in his own time, and in his own country, than *Newton* was in theſe kingdoms. With regard to others, *we are not to wonder*, as remarked by his eloquent Eulogiſt, *if philoſophers upon the firſt publication of the* Principia *took the alarm at the term* attraction, *as fearing the return of the* occult qualities; *or if, conſidering the difficulty of the ſubject, and the few words employed in explaining it, they wanted time fully to comprehend it* *. Theſe obſtacles have been removing by degrees, and the way at laſt has been ſo effectually cleared, that the name of *Newton* is not perhaps held in more eſtimation here, nor his principles more cordially embraced, than in thoſe very ſocieties of the learned, abroad, which at firſt ſhewed moſt unbelief, and at whoſe converſion therefore we ought moſt to rejoice.

The Royal Academy of Sciences, whilſt in an uncertain ſtate between the old and new ſyſtem of philoſophy,

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* Eloge de Newton, par M. de Fontenelle.

having, for one of the deciſive experiments, meaſured ſome degrees of latitude upon an arch of a meridian paſſing through Paris, and compared this menſuration with others, inferred the earth to be a ſpheroid with the longeſt diameter paſſing through its poles; but ſenſible that this operation had not been ſo unexceptionably conducted, as to ſatisfy either the followers of *Newton* or thoſe of *Huygens*, who both required a ſpheroid flattened at the poles, reſolved upon a farther and more accurate trial. With this view, in the year 1735, ſome choſen members from that illuſtrious Body were ſent to the polar circle, and others to the equator; at which places the differences of degrees being greater, the point in diſpute might be determined with leſs danger of error. How much to the honour of *Newton* and *Huygens* the reſult was, is ſufficiently known. All that is neceſſary to be mentioned here, is that in the year 1738, whilſt the academicians were ſtill in Peru, it occurred to M. *Bouguer*, one of that number, to put the *Newtonian* ſyſtem to another teſt, by inquiring into the attraction of mountains. This idea, which was originally from *Newton* himſelf, M. *Bouguer* communicated to his collegue M. *de la Condamine*, who readily aſſiſted in making the trial[*]. Thoſe gentlemen were perſuaded, that if the whole maſs of the earth were really poſſeſſed of ſuch a property, a high mountain, ſuch as nature had abundantly provided in that country, would ſhew ſome proportionable degree of it. That the largeſt of the *Andes* was indeed but a ſmall object in compariſon of the earth; nevertheleſs they reckoned, by a rough computation, that the attraction of *Chimboraço*, which they deemed the beſt for their purpoſe, would be equal to about the 2000th part of the attraction of the whole earth. Now, here the mountain acting as one, whilſt the earth as 2000, the direction of gravity would be viſibly turned out of the vertical line, for as much as this direction would be 1′ and 43″ towards the mountain. But how was this deflexion to be eſtimated? Only by finding the quantity of deviation of the plumb-line from a vertical poſition, by means of ſtars. In order to attain this point, they found it moſt convenient in their preſent circumſtances to take the diſtance of ſeveral ſtars from the *zenith* at two ſtations, one on the ſouth ſide of *Chimboraço*, and the other a league and a half to the weſt; that is, at ſuch a diſtance from the firſt ſtation, as that the plumb-line ſhould be but little affected by the mountain. This diſpoſition being made, they proceeded to their operations, of which we have a full and clear account by M. *Bouguer*, in his valuable treatiſe intitled *Figure de la Terre*; but of M. *de la Condamine*, we have only a ſhort abſtract of the narrative he preſented to the Academy; which abſtract is contained in his curious *Journal of his Voyage to the Equator*.

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[*] Bouguer, Figure de la Terre, ſect. 7. De la Condamine, Journal du Voyage à l'Equâteur.

From both it appears, that though thoſe learned perſons, during the time employed in this experiment (which the inclemency of the air, at that height in the atmoſphere, forced them to make very ſhort) I ſay, though during this time they ſpared no pains, yet their obſervations not only varied from one another, but ſeemed to be little ſatisfactory to themſelves. M. *Bouguer* ſays, that inſtead of 1′ 43″, which the plumb-line ought to have declined from the true vertical line, the total declenſion amounted only to ſeven ſeconds and a half: an effect that fell far ſhort of the expectations of a *Newtonian*. But thoſe candid gentlemen take notice, that, *as on one hand we are ignorant of the denſity of the internal parts of the earth, which may be conſiderably greater than what appears by its ſurface; ſo, on the other,* Chimboraço, *which they believed likely to be as ſolid as any other parts of the ſurface of the earth, might nevertheleſs* *in many places be hollow.* Nay, M. *de la Condamine* tells us, *that he was afterwards informed of a tradition in the country, that this very mountain had once been a volcano*; and adds, that *whilſt he and his collegue were about their experiment, they had actually found ſome calcined ſtones upon it.* From which circumſtances he infers, that *if one cannot juſt draw from this trial an abſolute proof of the Newtonian attraction, one can far leſs form any concluſion againſt it.* M. *Bouguer* goes farther and obſerves, that *if we will be ſatisfied with the bare fact, it is certain from this experiment, that mountains do act at a diſtance, but that their action is much leſs than what might be expected from their bulk.* He concludes his account in the true ſpirit of a philoſopher, by ſaying, that *as in France, or in England, a hill may be found of a ſufficient height for the purpoſe, and eſpecially if the obſerver would double the action, by making a ſtation on each ſide; he ſhould be happy to hear, on his return to Europe, that the experiment had been repeated, whether the reſult tended to confirm his obſervations, or to throw ſome better light upon that inquiry.* If the Society have fulfilled the views of that worthy man, who thus called upon them, we have to regret that he did not live long enough to ſhare the ſatisfaction with us.

I come now to Mr. *Maſkelyne's* labours, upon which I ſhall not expatiate, as I have already taken up too much of your time, and as I judge it unneceſſary to dwell long upon that part of my ſubject, which you have ſo lately heard in his own words, and which you will have in a few days publiſhed at large in your Tranſactions.

I need only remind you, that the zenith diſtance of a ſtar on the meridian being obſerved at two ſtations under the ſame meridian, one on the ſouth ſide of a mountain, the other on the north; if the plumb-line of the inſtrument be attracted by the mountain out of its vertical poſition, the ſtar will appear too much to the north by the obſervation at the ſouthern ſtation, and too much to the ſouth by that at the northern ſtation; and conſequently the difference of the latitudes of the two ſtations will be found by theſe obſervations greater than it really is. And if the true difference of their latitudes be determined by meaſuring the diſtance between the two ſtations on the ground, the exceſs of the difference found by the obſervations of the ſtar above that found by this meaſurement, muſt have been produced by the attraction of the mountain, and it's half will be the effect of ſuch attraction on the plumb-line at each obſervation, ſuppoſing the mountain attracts equally on both ſides.

To perform this experiment, Mr. *Maſkelyne* made choice of the Mountain Schehallien in Perthſhire, in North Britain, of which the direction in length is nearly eaſt and weſt, it's height above the ſurrounding valley at a medium is about 2000 feet, and it's higheſt part above the level of the ſea is 3550 feet. As the greateſt attraction of the mountain was to be expected about half way up it's ſides (which happened conveniently for the purpoſe of the experiment to be pretty ſteep) two ſtations for an obſervatory were accordingly choſen, one on the north, and the other on the ſouth ſide of Schehallien. The inſtrument with which he obſerved the ſtars was an excellent ſector made by Mr. *Siſſon*; and Mr. *Maſkelyne* has related at large all the precautions he took both for adjuſting this inſtrument in the meridian at each ſtation, and for ſatisfying himſelf that the line of collimation remained unaltered. From obſervations of ten ſtars near the zenith, he found the apparent difference of the latitudes of the two ſtations to be 54″,6; and from a meaſurement by triangles, formed from two baſes on different ſides of the mountain, he found the diſtance of their parallels to be 4364 feet, which, in the latitude of *Schehallien*, viz. 56° 40′, anſwer to an arch of the meridian of 43″: this is 11″,6 leſs than that found by the ſector. It's half therefore 5″, 8 is the mean effect of the attraction of the mountain: and from it's magnitude, compared with the bulk of the whole earth, Mr. *Maſkelyne* diſcovered the mean denſity of the earth to be about double that of the mountain.

In the execution of this intereſting experiment, our worthy brother has not only exerted a patience and perſeverance, but a ſagacity and judgment which muſt ever redound to his honour. All doubts about an univerſal attraction muſt at laſt be terminated, and every philoſopher in that reſpect muſt now become a *Newtonian.*

If I have related but two experiments that have been made, the firſt by the *French* academicians, and the other by Mr. *Maſkelyne*, it is becauſe no more have come to our knowledge; nor do I believe that more have actually been executed. For if, in occaſional menſurations of degrees of the meridian in different parts of Europe, thoſe employed have found varieties ariſe in their meaſures that they could not otherwiſe account for, than from the attraction of the mountains among which they carried on their operations, and accordingly have referred thoſe irregularities to that very cauſe; ſuch conjectures we admit may be well founded, but the meaſurements whence they ariſe we cannot reckon among the experiments we now treat of.

But was not the doctrine of an univerſal attraction ſo fully demonſtrated by *Newton*, as not to require any farther proofs from experiments? Demonſtrated it was, but not to the conviction of every individual. True Philoſophy condeſcends to adapt her inſtructions to different capacities, and is as willing to inform by palpable experiments as by geometrical demonſtrations. But to ſay the truth, ſomething ſeemed wanting here for the ſatisfaction of even the more enlightened minds. Such we reckon thoſe were who firſt made the trial. And did not *Huygens* himſelf, one of the greateſt philoſophers and geometricians of his age, find difficulties about this principle, even after the publication of *Newton's Principia*? nor do we learn that the doubts of that great man were ever removed [*]. To ſay nothing of the celebrated *Leibnitz*, and his numerous followers, who to this day are either wholly unbelievers in attraction, or at beſt but ſceptics on that article.

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[*] Vid. Hugen. Diſſert. de Cauſ. Gravitat.

You have therefore, Gentlemen, the ſatisfaction to think that you have completed a great and acceptable work to the ſcientific world; and that, though this has been a coſtly experiment, your gracious PATRON, who ſo liberally furniſhed the means, will highly approve your expending his benefaction ſo much for the advancement of Natural Knowledge and for the benefit of the public; and will ſo much the more be diſpoſed to ſhew you the like favour on future occaſions.

But for thoſe who wanted no freſh proofs of the univerſality of attraction, they muſt ſtill partake of the advantages accruing from this experiment, as being not only the firſt that has been made, but the beſt that could be deviſed, for eſtimating the mean denſity of the earth. The operation in Peru was too imperfect for that purpoſe, and had the circumſtances of that trial been more favourable, yet the ſuſpicion of their mountain having been once a volcano, was a ſufficient reaſon for admitting no evidence from it in this part of our inquiry. But for Schehallien, as it's appearance was particularly rocky, and as ſeveral ſpecimens of thoſe rocks have been preſented to the Society, and acknowledged to be mineral ſubſtances that had never paſſed through fire, we may conſider that mountain as one of the proper patterns of the denſity of the ſurface of the earth.

Theſe, Gentlemen, are the fruits of the operations of Mr. *Maſkelyne*, during a reſidence of four months in a mean hut, on the ſide of a bleak mountain, and in a climate little favourable to celeſtial obſervations. To theſe inconveniencies, however, he ſubmitted with patience and complacency, as he went at your requeſt and in purſuit of ſcience. You have heard his chief concluſions; but permit me to add, that as this is a new mine opened in the field of nature, I am confident that theſe will not be the only productions; but that, as in all great and ſucceſsful experiments, there will be in the proſecution of this ſubject ſome valuable truths brought to light, of which at preſent we can form no particular conjecture. Mean while we have the pleaſure to find the doctrine of *univerſal gravitation* ſo firmly eſtabliſhed by this finiſhing ſtep of analyſis, that the moſt ſcrupulous now can no longer heſitate to embrace a principle that gives life to Aſtronomy, by accounting for the various motions and appearances of the Hoſts of Heaven.

MR. MASKELYNE,

THE judgment, Sir, of the Council, in awarding you the prize, having received the ſanction of the ROYAL SOCIETY, I do, in the name and by the authority of that illuſtrious Body, preſent you, their moſt worthy Brother, with this ſincere pledge of their affection; as the laſting token of their acknowledgment for your ſeveral ingenious and uſeful communications, and more particularly for this laſt painful and capital experiment, which adds no ſmall luſtre to their Tranſactions. And after expreſſing their grateful ſentiments for what you have already done for their ſervice, I would farther ſay, that they perſuade themſelves, from your talents, your love of your profeſſion, and your happy period of life, you will continue ſteddily to purſue that path which you have ſo early entered upon, and which ſo ſurely leads to great and uſeful diſcoveries. You have, Sir, in charge the nobleſt branch of Natural Philoſophy: ſuch it has ever been held by this Society, and as ſuch it ever has been cheriſhed and cultivated by them. And they flatter themſelves that their cares and ſolicitude have not been fruitleſs; ſince, from their firſt inſtitution to this day, there have never been wanting ſome excellent men in that line, to promote the ſcience, and do honour to this Community. But ſo tranſcendently great is that part of the creation, that though the Divine Author has vouchſafed, in theſe latter days, to open to the humble and patient inquirers into Nature the *Cauſes of Things*; yet we muſt ſtill cry with the ancient ſage, *Lo, theſe are part of His ways, but how little a portion is heard of them!* As much then remains to be explored in the celeſtial regions, you are encouraged, Sir, by what has been already attained, to perſevere in theſe hallowed labours, from which have been derived the greateſt improvements in the moſt uſeful arts, and the loudeſt declarations of the power, the wiſdom, and the goodneſs of the Supreme Architect in the ſpacious and beautiful fabric of the World.

FINIS.